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ESP AND MEMORY: A PHYSICAL THEORY*

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1 *Introduction*

IN this paper I propose to present a new theory in physics, and apply it to some problems in psychology.

The original aim of the theory was to explain extra-sensory perception (hereafter called ESP) in physical terms. For there is some good evidence for ESP,[1] which is rejected or ignored by many scientists because it is quite incompatible with current physical theory. ESP represents the transmission of information between two points without the intervention of a physical signal. The points may be separated by miles in space or days in time. I shall here consider only telepathy, the best-established form of ESP; I shall not take special note of clairvoyance, precognition, or psychokinesis. Physically, one must suppose that information in a person's mind corresponds to a particular pattern of activity in his brain; so that telepathy consists of the reproduction in one brain of a pattern analogous to that in another brain.

The difficulty of fitting ESP into quantum physics is reminiscent of the difficulty at the turn of the century of fitting the facts of blackbody radiation into classical physics. In both situations, the reigning theory is supported by vast numbers of accurate experiments, but cannot begin to explain the results of other newer experiments. The solution then was to form a theory which under classical conditions predicted classical results but under conditions involving few quanta predicted the new results. This presupposed that parameters were involved in the new laws whose values differed widely in the two sorts of experimental condition. In effect two domains were distinguished, each with its own peculiar phenomena.

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* Received 6. vii. 59.

[1] For some references see section 7 of this paper.

I believe that a similar manoeuvre can integrate ESP phenomena into current physics. The main difference between the conditions under which classical and ESP phenomena occur is one of complexity. All controlled physical experiments are performed on structures simplified as much as possible; whereas ESP phenomena have been found only in the human brain, the most complicated structure in the universe. Hence, predictions in the usual domain of physics will be left almost unchanged if any law is added of the form: ' Effect X is found in structures. The size of X is proportional to the complexity of the structure.' By complexity I do not mean mere size, or number of molecules involved; I mean the number and interrelations of the parts. A television set is more complex than a steam engine; an insect more complex than a star. Any theory of this form will be a holistic theory,[1] and will result in the distinction of a highly structured and a simple domain.

2 *The Theory of Resonance*

Telepathy, we have said, is the partial reproduction in one brain of a pattern in another brain. A general hypothesis to cover this would be: ' Any two physical structures which are similar, tend to become more similar.' To make the law correspond with the physics of simple structures, we must add: the strength of this tendency is proportional to the complexity of the two structures. To make it correspond with the facts of telepathy we must add: the tendency is independent of the separation of the structures in space and time, but increases with the similarity of the two structures.

We now have a hypothesis which formally resembles Newton's Law of Gravitation. It runs: ' Any two structures exert an influence on each other which tends to make them become more alike. The strength of this influence increases with the product of their complexities, and decreases with the difference between their patterns.' I shall call this the Hypothesis of Resonance because tuning forks and oscillating circuits are in some ways analogous. Their mutual influence depends as much on their intrinsic similarity as on their spatial proximity. But this is only an analogy, as the interaction is by means of a continuous causal series of events in one case, not in the other.

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[1] The term ' holistic ' here refers to laws about a complex whole which differ from those that would be predicted from a knowledge of its parts.

I shall call the influence spoken of in the hypothesis, an eidopoic influence. (Greek *εἰδοποιος*—creating a form or pattern.) It is analogous to the concept of force in the law of gravitation. Thus it is an intervening variable, and needs to be related to actual events by a second law. What effect will a given eidopoic influence have on a given brain? The amount of change in pattern produced must increase with the strength of the eidopoic influence. Simplicity requires that the change in pattern shall be a change which is allowable by quantum mechanical laws. Hence in quantum physics it had a calculable probability of occurrence. The principle of correspondence with current physics leads to the postulate: the smaller the probability of occurrence of a given transition, the greater the eidopoic influence needed to produce it. The hypothesis now runs: 'The change of pattern produced by an eidopoic influence increases with its strength, and with the quantum mechanically defined probability of the change.' This hypothesis is formally analogous to Newton's Law of Force. I shall call it the Hypothesis of Eidopoic Influence.

These two hypotheses together constitute the preliminary statement of the theory. They predict significant deviations from current physics only under two conditions:

(i) Complex structures must be involved.

(ii) These structures must have 'indeterminate' features in their observable behaviour.

This criterion needs some explanation. If a given structure (a steam engine, a spinning roulette wheel, a reacting chemical mixture) is in a state where all possible transitions have probability very near 0 or 1, then only a very large eidopoic influence could affect its behaviour. Only an occurrence which had an intermediate probability (say from 0·1 to 0·9) could be appreciably modified by an eidopoic influence of given strength. This quantum indeterminacy occurs in certain aspects of some simple systems: the time of radio-active decay of nuclei, the position at which an electron strikes a screen, or the time at which a super-cooled liquid will crystallise. I shall later show that it also happens in the response of nerve cells to a near-threshold stimulus, and argue that this is important in the function of the complex system which is the brain.

Thus these two conditions, complexity and indeterminacy, have so far been satisfied only in the brain. But a physical apparatus could also be built which would satisfy them both, and provide a controlled test of the theory.

General Comments. There are now several tasks awaiting us:

(i) to substantiate the comments just made about the brain.

(ii) to consider in more detail the relation of the theory to quantum physics.

(iii) to show that the theory actually does shed light on ESP. It will also be shown that it illuminates the physiology of perception and memory.

These will be considered in later sections of the paper. But first, a few comments on the general nature of the theory.

The theory here presented is holistic and physical. For a physical theory is a falsifiable set of predictions about the behaviour of atoms, photons, or the like. And a holistic theory is one which implies that the behaviour of a whole cannot be calculated accurately from the laws observed to govern the behaviour of the separated parts. The present theory is therefore much more holistic than current quantum physics, in which few ' emergent wholes ' appear. There have been equally holistic theories in philosophy, religion, biology, and psychology; but not in physics. This is a gap, as a holist of any other sort needs also to be a physical holist, or lapse into radical dualism or inconsistency.

The theory as presented in this paper makes only qualitative predictions. This is for two reasons: (i) No mathematical machinery yet exists to deal with structures as here envisaged. The characteristics required of a suitable mathematical formalism will be outlined later. (ii) No controlled experiments have been done in this field. The only experiments to date concern human brain function, which is influenced by thousands of irrelevant factors. Accurate results could be obtained only by experimenting on a specially built apparatus, rather than on the brain.

One may intuitively dislike the present theory because of its novel features. It postulates action at a distance in space and time, and co-ordinated action at many points in a structure. It postulates that some, at least, of the ' indeterminacies ' in quantum physics are in fact determined in a holistic way. All this may go against the grain. Physicists had similar feelings when quantum theory was introduced; many still dislike its wave-particle duality. The main virtue there, as here, was not the attractiveness of the model (or lack of a model), but the unity and explanatory power of the theory.

However, one must have some respect for established usage. A determinant of an event is any factor which has played a part in shaping it. But the only determinants so far employed in physics have been 'physical causes', which have traditionally had the following properties:

(i) A transmission of energy or momentum is involved.
(ii) The cause acts before the effect.
(iii) There is a continuous chain of intervening events from cause to effect.

These three have been slightly modified by the causes used in quantum physics. But here they are altogether discarded. So I have called these determining influences not 'causes' but 'eidopoic influences'. Both causes and eidopoic influences, being determinants, can lead to predictive laws, but differ in the three ways mentioned above. There are many causal relationships; but in this paper only one kind of eidopoic influence is discussed, which is that of resonance.

The present theory is no more and no less mechanistic than quantum physics. The mechanistic ideal used to imagine a Laplacean intelligence who could be given a complete description of the state of the universe at any one time and who could then make predictions as to its state at all future times. This ideal is partially fulfilled in quantum physics; complete fulfilment is rendered impossible by the uncertainty principle. In the theory of resonance the situation is the same, though the actual predictions made are different. Either theory therefore is compatible with full mechanism, a teleological view, or some other non-mechanistic view such as that of Bohm.[1] The theory is not a theory of consciousness or free-will, and no more implies the existence of mind than gravitational forces imply the existence of an ether.

I shall now turn to more detailed considerations.

3 *Quantum Indeterminacy in the Brain*

The central nervous system may be considered as a machine with many inputs which affect its pattern of functioning. Some of these inputs are via afferent neurones from sense organs or proprioceptors. Some are chemical: the presence in its blood supply of drugs, hormones, or of unusual concentrations of metabolites. It will now be argued that there is a large input from the quantum level. This would normally be called 'entropy' or 'noise', but on the theory of resonance some of it represents 'information'. That is, the quantum level input is undetermined by the present laws of physics, and so can be determined by this other law.

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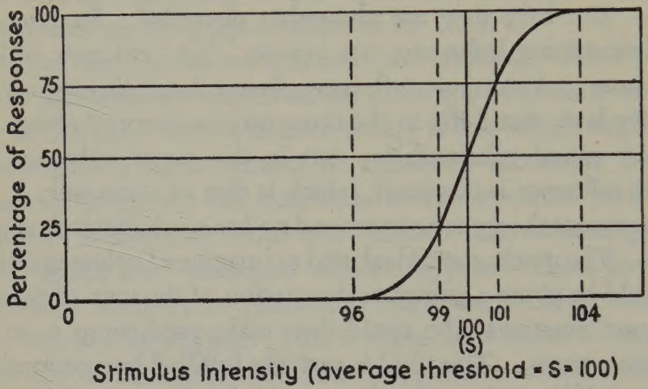
[1] D. Bohm, *Causality and Chance in Modern Physics*, London, 1957, pp. 130–170

The case for quantum indeterminism in the response of a single neurone is strong.[1] Experiments on an isolated nerve fibre showed that its threshold was not constant, but fluctuated according to a definite statistical law. The frequency with which it responded to various stimuli was as shown in the accompanying graph.[2] If a stimulus S caused it to fire 50 per cent of the time, then the neurone showed unpredictability in responding to stimuli in the range 96 per cent S to 104 per cent S.



Percentage of Responses of a Neurone to Stimuli

This variation in threshold is believed due to the fluctuation in concentration of metabolites near the nerve cell membrane. A mathematical theory on these lines has explained the observed results in detail. These fluctuations in local ionic concentration are 'indeterminate' quantum level phenomena. Thus any neurone stimulated to a point near its average threshold will show quantum level indeterminacy in its behaviour at that time, and may or may not respond.

How important are these facts in the functioning of the whole brain? How often are neurones stimulated to within the indeterminate range, and how much effect does this have on brain function? A very rough estimate can be made based on three assumptions:

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[1] For a full account see N. Rashevsky, *Mathematical Biophysics*, Chicago, 1948, pp. 312 ff.

[2] Adapted from N. Rashevsky, op. cit.

(i) Assume that the experimental figures apply roughly to all neurones. This is likely to be true. Thus if a neurone during any short period be stimulated so that the probability of its firing lies between ¼ and ¾, the stimulus must have been between 99 per cent S and 101 per cent S. Let this be defined as a marginal state of the neurone.

(ii) Suppose that the threshold stimulus S is received by a neurone when 1/20 of its afferent synapses are fired roughly simultaneously. This is reasonable. A neurone may have up to a few hundred synapses on it. And not fewer than ten synaptic stimuli are needed at any one time to fire known types of neurone. Second-order effects of temporal summation, etc., are here disregarded.

(iii) Suppose that, for a cortical neurone within any five millisecond period, all values of stimulation are equally likely. (Five msec. is chosen because it is the approximate range of effectiveness of temporal summation.) This assumes a random connection of neurones, and so will be true only on the average. Also it implies that in each 5 msec. period a cortical neurone has about a 19/20 chance of being fired; allowing for refractoriness, it will therefore fire about 100 times a second. Thus the following estimate will apply only to active parts of the brain. (States of lesser activity (e.g. relaxation, trance, light or deep sleep) will show a reduced average stimulation per neurone. In fact, the estimated maximum proportion of marginal neurones will occur in some state of lessened brain activity.)

From these assumptions, a neurone will be made marginal by a stimulus in the range 99 per cent S to 101 per cent S, a range of 2 per cent S. But the total possible range of stimulation is 20 S. Hence, at any moment its chance of being stimulated to within the marginal range is 1/1000. Since there are about 10^{10} cortical neurones, this means that at any moment there are about 10^7 marginally stimulated neurones in the brain. Of course, they are not always the same neurones.

By comparison there are about 10^6 neurones in the optic nerve. Thus, the quantum level input to the brain is at least ten times as large as the visual input. An eidopoic influence strong enough to use all marginally stimulated neurones would have this very large channel capacity open to it. Of course, eidopoic influences of this strength may not exist in nature; the owner of a powerful wireless set has no guarantee that there are wireless programmes to receive.

Even if the above estimate were in error by a factor of 100, it would not invalidate the qualitative argument. This concludes this section, except for two digressions.

Firstly, the theory of resonance is quite compatible with theories of free will or of human purposive action, though not directly concerned with them. For such a theory, one would postulate that there are two forms of eidopoic influence, namely resonance and human volition; or that volitions could modify the strength of resonances. The primary diathetes of Kapp[1] would then be the ions which influence the neural thresholds. The postulation of free will or of diathesis is, in fact, much easier if correlations like those in the theory of resonance are also postulated. Eccles, a theorist of free will, points this out: 'This hypothesis assumes that the " will " or " mind influence " has itself a spatio-temporal patterned character in order to allow it this operative effectiveness.'[2] But to pursue this would be outside the scope of this paper.

Finally, it seems likely that quantum phenomena play a part in biological processes generally, though a much smaller part than in the brain. I. D. London argues this.[3] He believes that this indeterminism represents mere meaningless noise, but on the present theory these phenomena would obey the law of resonance. There is thus a possibility that some holistic processes occur in general biology. This too is outside the scope of this paper.

4 *Relationship to Physics*

In this section I shall trace how the theory of resonance is logically related to non-relativistic quantum mechanics. In quantum mechanics it is convenient to distinguish between the actual results of experiments, the mathematical laws which predict future results, and the philosophical interpretations placed on the laws. Controversy about the physical reality of waves or particles is irrelevant, whereas argument about determinism is highly relevant.

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[1] R. O. Kapp, *Mind, Life and Body*, London, 1951, pp. 156-160, 181, and throughout the book

[2] J. C. Eccles, *The Neurophysiological Basis of Mind*, Oxford, 1953, pp. 277-279

[3] I. D. London, ' Quantum Biology and Psychology ', *Journal of General Psychology*, 1952, **46**, 123-149

Indeterminism. It is found that under identical conditions a given system (e.g. an electron) may behave differently on different occasions. It follows that its behaviour is not fully determined by the physical conditions of the experiment; that is, by the system's physical surroundings and its ' state '. Several further views are possible.

(i) There are no further determinants of its behaviour. This philosophical indeterminism is common today, but was objected to by Einstein.

(ii) There are further determinants. These have been supposed to be, for example:
   (*a*) Hidden variables existing at a sub-quantum mechanical level. Bohm has suggested this.[1] These might become important at very high energies and short distances, but are not observable in the low energy domain. Hence they are irrelevant to organisms.
   (*b*) Processes such as free will or diathesis—taking place in the brain or by psychokinesis. The theories of Kapp and Eccles are of this type.
   (*c*) Holistic aspects of structured situations, as in the theory of resonance.

These last three models are all perfectly tenable on general grounds, as are combinations of them. Objections can be made to them only from the statistical laws of quantum mechanics.

Quantum Statistics. Experiments show that although individual quantum events are physically undetermined, yet assemblages of these events form definite statistical patterns. These results, at least in the low energy domain with which we are concerned, are well described by the mathematical laws of quantum mechanics. The laws predict the mean values of observables, their mean fluctuations, and various correlations between observed quantities (e.g. position and momentum). Moreover, von Neumann proved that this formalism is complete. No new law can be added unless some old law is changed.[2] Alternative interpretations of these facts are possible.

(i) Current quantum mechanics exactly describes all physical systems of low energy. No further laws can be added.

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[1] Bohm, op. cit. pp. 106 ff.

[2] For an account see Ibid. pp. 86, 95-96

(ii) Current quantum mechanics is a very good approximation, but is inexact in some ways. Then further laws, i.e. further correlations between the variables, may be added. These could have remained unnoticed for several reasons:

  (*a*) The new laws nowhere differ greatly from the old ones. This case is unimportant for our purposes.

  (*b*) The correlations only become marked under conditions which have not been used in experiments. Schrödinger's quantum theory cannot be extrapolated successfully to the relativistic domain; nor, I suggest, to the domain of the highly structured. This has already been discussed.

  (*c*) The correlations are with observables which were not considered to form part of the experimental context. Just as apparently arbitrary variations in the weather in fact depend on sunspots, so seemingly random quantum level events may correlate with distant structures or with human volitions.

  (*d*) The correlations might be too complex to have been noticed. There are an infinite number of possible types of correlation, and one can test for only a few. For example, the first 2000 digits of π are negative to the usual statistical tests for order; yet they are related, by the more subtle rule which is the rule for computing π. Observers of complex structures would be likely to find complex ordering only of the types they were specially looking for.

Thus, if the theory of resonance holds there are the above three reasons why its effects would probably not have been observed. Quantum statistical laws would still seem to constitute the whole truth. Hence their apparent complete success is no argument against the truth of the theory of resonance. The theory of resonance does no harm in physics; it remains to show that it does much good in psychology.

5 *A Possible Quantitative Approach*

This section is not absolutely essential to the argument. Its purpose is twofold: to clarify the concepts of the theory of resonance, and to make the theory as nearly mathematical as possible. For no suitable mathematical formalism to deal with complex structures exists. And while particular quantitative results in the theory of resonance might be obtained, no general theory could be stated without using such a formalism. So I have given lines along which one might be constructed. Of course, it is conceivable that the straightforward approach given here might fail but that some other mathematical approach should succeed. I have expressed the present approach in the form of postulates.

Definition. Structure. Any set of physical entities. They need not be contiguous in space, but must exist at the same (not sharply defined) time. (This is thus a non-relativistic theory.)

Definition. Pattern. A description of the form taken by a structure at any one time. This description includes both spatial and causal relationships. Thus a structure s might alter from pattern A to pattern B. In quantum mechanics, for example, the state or pattern of an n-particle system at any (not sharply defined) moment is represented as a wave in $3n$-dimensional space.

Postulate 1. Corresponding to any pattern A, a scalar quantity $c(A)$ can be defined which is a measure of the *complexity* of the pattern. We require this to satisfy:

(i) $c(A) \geqslant 0$.
(ii) If A and B do not interact, $c(A+B) = c(A) + c(B)$.
(iii) If A and B interact, $c(A+B) > c(A) + c(B)$.

Postulate 2. Corresponding to any two patterns A and B, a scalar quantity $d(A,B)$ can be defined which is a measure of the unlikeness of or *difference between the two patterns.* We require that this quantity should formally resemble 'the distance between two points', so it must satisfy:

(i) $d(A,A) = 0$
(ii) $d(A,B) = (B,A)$
(iii) $d(A,B) + d(B,C) \geqslant d(A,C)$
(iv) Given A, B, and $k(>0)$, there is one and only one X satisfying the equations $d(A,X) + d(X,B) = d(A,B)$ and $d(A,X) = k.d(X,B)$.

These four axioms are also the axioms governing the distance between two points in a metric space. Thus they imply that patterns can be exhibited as points in an abstract 'pattern space'. This space would have to have an indefinitely large number of dimensions, depending on the number of particles in the structures being considered. (The image of such a 'pattern space' seems to underlie metaphors like: 'he was not far from the truth.' Here it is presented as a definite mathematical idea.) In this representation, structures as they alter describe paths in pattern space. Resonances appear as attractions between structures in pattern space; like vectors, they have direction and magnitude.

From this, possible laws of resonance could easily be formulated. If the eidopoic influence of a structure at B on a structure at A is $\bar{E}$, then $\bar{E}$ is an attraction along the line AB. E, the strength of $\bar{E}$, would have to satisfy:

(i) E is small when $c(A)$ or $c(B)$ is small.
(ii) E is small when $d(A,B)$ is large.
(iii) E is large when $c(A)$ and $c(B)$ are large and $d(A,B)$ is small.

Many functions would satisfy these requirements; for example,

$$E = k \cdot \frac{c(A)\,c(B)}{[d(A,B)]^2}$$

Experiments would be required in order to select the right function.

The final problem would be to give mathematical form to the law of eidopoic influence. The following approach seems the most promising. If a structure is at point A in pattern space, then at a later time t it will be at X. Quantum mechanics will define the probability $P(X,t)dX$ of its then being in a small volume element dX surrounding X. Being a well defined probability function, it will satisfy:

(i) $P(X,t) \geqslant 0$, i.e. the probability is nowhere negative.
(ii) $\int_{-\infty}^{\infty} P(X,t)dX = 1$, i.e. at any time t the structure is certain to be somewhere in pattern space.
(iii) $P(X,t) \to 0$ as $d(A,X) \to \infty$, i.e. the structure alters at a finite rate.

Now let us suppose that any eidopoic influence $\bar{E}$ acts as a bias on the quantum mechanical probability function $P(X,t)$. It will thus define a 'corrected probability function' $\Pi(X,t)$. $\Pi(X,t)$ will be a function of $\bar{E}$ and of $P(X,t)$. It too must satisfy the three conditions just mentioned. It must also satisfy:

(i) $\Pi(X,t) \to P(X,t)$ as $\bar{E} \to 0$.
(ii) $\Pi(X,t) \to 0$ as $P(X,t) \to 0$.
(iii) $\Pi(X,t) \to 1$ as $P(X,t) \to 1$.
(iv) $\Pi(X,t) - \to P(X,t)$ increases when E_x increases. (Where E_x is the component of $\bar{E}$ in the direction AX.)
(v) Let $\int_{\Gamma} P(X,t)dX$ be the quantum mechanical probability of finding the structure in the region Γ at time t (for example, the probability of finding that a neurone had passed into one

of the states equivalent to its having fired). Then, if $\int_\Gamma P(X,t)dX$ is marginal (i.e. has a value between $\frac{1}{4}$ and $\frac{3}{4}$, say) we require that even a small E_x can make $\int_\Gamma \Pi(X,t)dX$ approach closely to either 0 or 1. (i.e. marginal neurones must be easily controllable by eidopoic influences).

So we have finally:

Postulate 3. *A corrected probability function* $\Pi(X,t)$ can be defined which satisfies the conditions just given.

Thus structures in pattern space might become aggregated into groups (i.e. assume similar patterns), or describe orbits. It would depend on the exact initial conditions and on the exact form of the laws adumbrated here.

6 *Applications: General*

So far the discussion has been in terms of physics and brain physiology. Now the laws are to be applied to psychological problems. For this transition, the working hypothesis is that similar psychological states correspond to similar patterns of neural activity. (This is merely an extension of the doctor's belief that similar pains have similar physical causes, or anyone's belief that similar facial expressions are associated with similar feelings. It does not imply that the neural patterns 'look like' the experiences. Nor does it imply anything about the nature of the mind or consciousness.)

The brain can be regarded as a pattern of neural activity or as a pattern of molecular activity. The neural description is a condensed version of the molecular description. The theory of resonance has been postulated in terms of patterns viewed at the molecular level. But if two molecular patterns resemble each other, then the corresponding neural patterns will also tend to resemble each other. This is the useful form of the law for psychology. Because most neural activity is not associated with consciousness, resonance applies chiefly at an unconscious level; but its products may become conscious.

There are possible applications of the theory of resonance to nearly every activity of human beings. These range from the very vague to those with much experimental evidence to support them. I shall in the next three sections consider some concrete applications, to three problems rich in experimental facts but poor in convincing explanations. These are the problems of telepathy, memory, and perception.

But first it is tempting to trace some rather less well documented connections, both for their intrinsic interest and because this may lead to their becoming better documented. By means of the theory of resonance, a neat distinction can be drawn between structural mental disease (due to physical injury to the brain), functional mental disease (due to the formation of injurious resonances and habits), and perversity (due to injurious volitions). What is more, it provides a lucid model of how these three can be interrelated in a single patient. (For lack of such a model, believers in one of these three forms of mental abnormality have often been unable to accept the evidence that the other forms exist. When discussing the causes of abnormal behaviour, endocrinologists, psycho-analysts, and schoolmasters cannot easily find common ground.)

Again, a resonating structure would show goal-seeking behaviour. That is, it would tend towards the same final state when in any one of a variety of initial states. It would be more flexible than reflex or homeostatic mechanisms; for it would respond more to generalised patterns than to exact details of the stimulus. It would be more 'intelligent' in its methods than trial-and-error mechanisms like Ashby's homeostat.[1] So the resonance concept enriches the armoury of the cyberneticist. If present computing machines can 'think logically', then one which embodied a structure capable of resonance could also 'think intuitively'.

Finally, it is possible to suppose that at an undifferentiated and deeply unconscious level people's brains are in resonance much of the time. This would give a basis for theories of the collective unconscious, group mind, participation mystique, or racial memory.

7 *The Problem of Telepathy*

In spite of the difficulties this raises, the existence of telepathy is unquestionable. The mention of one investigation will suffice, that of Soal on Shackleton.[2] Basil Shackleton scored consistently above chance level in card guessing experiments over a period of several years. He sat in one room writing down his guesses, while the agent looked at cards in another room. Different experimenters controlled percipient and agent. Shackleton scored highly with only three out of eleven agents who were tried. Whether he knew of the change or not, his score was above chance level only when the agent actually looked at the cards. His total score gave odds against the chance hypothesis exceeding 10^{35} to one. Many outside observers witnessed these experiments, and every suggested normal explanation was excluded.

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[1] W. R. Ashby, *Design for a Brain*, London, 1952, pp. 93-102

[2] S. G. Soal and F. Bateman, *Modern Experiments in Telepathy*, London, 1954, pp. 132-198

To attempt a critical review of the phenomena observed in such experiments would be a formidable task. There are many such experiments: in some the conditions were watertight, but in others they were not. Some gave highly significant results, others did not. And many obscure but statistically significant effects have appeared whose interpretation has hardly begun (e.g. the decline effect, positive and negative displacements, and various position effects[1]). But the following generalisations seem justified.

Telepathy is usually a fairly erratic process. It is unconscious: the subject does not know which, if any, of his guesses are correct. Moreover, telepathic information may undergo distortion before reaching consciousness. Some subjects, often hostile or bored ones, score very significantly below chance level—which seems to be repression of the information. Experiments with freer material such as drawings show that subjects may seem to produce many associations to the target drawing, without ever consciously reaching the target.[2] The faculty is not under voluntary control and is highly labile. Only a few subject-agent pairs have produced consistently high scores. But telepathy can occur over a distance of 200 miles,[3] and over a time interval of a day or two in either direction.

All these facts can be interpreted fairly simply by the theory of resonance. Resonance of the brain with other similar structures will result in the mixing of resonance information into all sensitive neural processes. Much of this resonance will be with other brains, i.e. telepathy. We can therefore consider telepathy to be resonance between two brains. Thus any person is theoretically capable of telepathy. There are two reasons for the seeming rarity and capriciousness of telepathy.

First there will be difficulties in forming the right resonance. If the brain is resonating about equally with many other structures, the many inputs of information will interfere with each other, and much of the information will be corrupted. The ideal conditions will be when the brain is in strong resonance with one other brain, and in weak resonance with all other structures. This would be more likely to happen with brains which were unusually similarly built. This might explain why telepathic percipients can succeed only with certain agents. It is noteworthy that the best telepathy on record is between two cousins.[1] Better telepathy still might be obtained between identical twins.

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[1] S. G. Soal and F. Bateman, *Modern Experiments in Telepathy*, London, 1954, pp. 309-337

[2] D. J. West, *Psychical Research Today*, London, 1954, pp. 136-7

[3] S. G. Soal and F. Bateman, op. cit. pp. 284-294

Telepathy would also be more likely to occur connecting times when the activity patterns in the two brains were unusually similar. (These times might not be simultaneous in the two brains. My thoughts on Tuesday might resemble yours on Friday.) This would tend to happen more among close friends. It would tend to be stimulated by mutual preoccupations, or by happenings that chanced to evoke similar sets of associations. It might be increased by concentrating on the structure of the target, as in experiments. Much would happen in an untraceable way. But it is clear that the conditions favouring ESP are similar to those favouring sensory communication or parallel trains of association. Hence, planned experiments which eliminate these factors probably detect only a fraction of the ESP which normally accompanies perception without being noticed.

Secondly, there are difficulties in abstracting the telepathic information from the complex pattern of brain function in which it is embedded. These difficulties also occur in perceptual processes. A non-musical adult listening to a symphony cannot separate the sounds of the different instruments, nor recognise the themes when they recur in an altered form. Quite possibly ESP ability requires this kind of skill in unemotional situations such as the usual planned experiment. But more emotionally charged material might influence most of the contents of consciousness, so that discrimination would hardly be necessary. Thus many people report a single spontaneous telepathic experience, such as the premonition of a serious event affecting a relative. In a similar way, in planned experiments much depends on a friendly atmosphere. More success has been obtained when the subject is ' on his mettle ', or stands to gain some reward if he is successful. People often score better with the symbols which have most psychological significance to them. Decline effects in long series of experiments can be attributed to boredom, so that the ESP material no longer has the emotional charge to become readily conscious.

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[1] S. G. Soal and H. T. Bowden, *The Mind Readers*, London, 1959. The whole book concerns experiments on two Welsh boys.

8 *The Problem of Memory*

In a lifetime, it is estimated, the human memory stores 10^{13} to 10^{15} ' bits ' of information. No plausible account has been given of brain mechanisms which could do this. Suggested methods of storage in the brain have been by reverberating circuits, synaptic changes, or even changes in the protein molecules within the nerve cells. None of these methods is adequate for the prolonged storage of complex memory patterns: such as a clear memory of a day spent with a family one met only once twenty years ago.[1] I shall argue that immediate memory is by means of reverberating circuits, and that simple motor habits may be stored as synaptic changes; but that the only conceivable explanation of a long-term complex memory is the unconventional one of a brain's resonance with a past state of itself.[2]

Three general objections arise to the theory that remembering is the reactivation of a ' trace ' which the experience left on the synapses or other objects in the brain:

(i) There are not enough synapses to store all memories. And if smaller objects are suggested, such as protein molecules, then if their constitution can be changed by neutral action it is unlikely to be stable against metabolism and random ' noise '.

(ii) A complex thought or experience involves the activity of millions of neurones. Any memory trace left would involve as many neurones. And each neurone must carry traces, from thousands of millions of memories. To recall a memory would involve the simultaneous selection of each bit of its trace from the appropriate neurone. This vast feat of co-ordination has no conceivable mechanism in current physics.

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[1] A similar conclusion is reached in W. M. Elsasser, *The Physical Foundation of Biology*, London, 1958, pp. 133-144. He holds that ' the maintenance of information in the organism is not, in general, effected by mechanistic means ' (p. 122).

[2] The view that the past can affect the present directly is found in Russell's theory of mnemic causation (B. Russell, *The Analysis of Mind*, London, 1921, p. 78) and in Bergson's theory of recollection (H. Bergson, *Matter and Memory*, London, 1911, pp. 191-195). However, the present theory differs from these two theories in many ways.

(iii) Lashley's Law of Mass Action. Cortical lesions cause a memory loss whose size depends on the size of the lesions but not on its position. The memory loss is greatest for complex memories. What is more, after a time the memories may return almost completely. So the same neurones are not always involved in the recall of a given memory.[1] No wonder that Lashley remarked, 'I sometimes feel, in reviewing the evidence on the localization of the memory trace, that the necessary conclusion is that learning just is not possible.'

The rôle of reverberating circuits is interesting. From introspection we know that images may persist for seconds and thoughts for minutes. But any nerve impulse lasts only about two milliseconds. So these persistent patterns must be represented in ever changing neural elements, like ripples on a lake. The brain could do this fairly easily by means of the many reverberating circuits which it does in fact contain. Immediate memory may well be in terms of reverberating circuits. This has the facts of retrograde amnesia to support it. Brain reverberation fades away in sleep and anaesthesia, and is blotted out by epileptic fits and electric shock treatments; all these cause loss of the memories of the previous few seconds or minutes. But after a few minutes, it seems, memories have become fixed in some other form. So long-term memory cannot be explained in terms of reverberating circuits.

Eccles has shown that changes occur in a synapse that suffers prolonged hard use.[2] These changes might become permanent; if so, they would be a basis for the formation of habits. For conditioned reflexes and simple habits, much more than other sorts of learning, resemble fixed connections established between the two definite and unchanging sets of neurones concerned with stimulus and response. Complex memories, for reasons already given, cannot be explained in this way.

The theory of resonance memory thrives on precisely the facts which are stumbling blocks for the conventional theories of memory. Memory requires the approximate reproduction in the brain of an earlier pattern of activity. This can occur by resonance; the more complex the memory pattern, the greater the influence of resonance. No storage mechanism is required, and there is no fading with disuse.

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[1] C. T. Morgan and E. Stellar, *Physiological Psychology*, New York, 1950, pp. 485-487, 505-507

[2] Eccles, op. cit. pp. 203-211

Moreover, a resonance influence is not tied to any particular neurones; memories could recur anywhere in the cortex. This explains the Law of Mass Action. A pattern in any part of the cortex which resembled a previous pattern anywhere in the brain, would be drawn to resemble it more. Thus, one would pass from a given idea or image to a related one—a well known law of association. Complex memories can thus be explained as resonance with the past states of one's brain.

The concept of resonance memory beautifully complements the concept of reverberation memory. A pattern which has reverberated in the brain for minutes or hours will be a much better target for subsequent resonance than if it had made only a momentary appearance. (This fits the facts of learning and of retrograde amnesia after concussion.) On the other hand, reverberating circuits are essential to the building up of a resonance memory pattern in the brain. This can be seen as follows. Suppose that a given image is present to a person, and then gives way to another by resonance memory. This must occur by effects on the neurones in the reverberating circuits which are marginal—i.e. on about 1 in 1000 of them. So the pattern will change gradually towards the memory rather as successive drafts of a speech may gradually approach the desired form. (As the neurones in the pattern change about every 2 msec., all of them would have been marginal in about two seconds. So an association whose pattern was different by 25 per cent could occur by resonance in a minimum of half a second. This seems about right.)

Also, the concept of resonance memory complements the concept of synaptic memory. Resonance can provide the co-ordination to unite bundles of reflexes into complex and plastic habits like driving or speaking a foreign language. And even very simple habits are probably not pure stimulus-response connections; their formation usually depends on motivation and the whole context as well as on the supposed stimulus. In another sphere, trains of thought seem to be a combination of resonance and habit forms of memory. The recall of a mathematical proof usually depends both on established habits of thought and on more complex single memories. If thought is a structure, its bricks are often habits; but its design is due more to intuition.[1]

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[1] This resembles Polanyi's distinction between the tacit and articulated components of thought. M. Polanyi, *Personal Knowledge*, London, 1958, pp. 69-131 *et passim.*

The resonance explanation of memory is like the resonance explanation of telepathy, but there are interesting differences to be noticed. Memory is much more reliable conscious and voluntary than ESP. Why should this be? Two factors may be responsible.

(i) A brain pattern is much more likely to resemble a previous pattern of its own than a pattern in some other brain. For each brain is different. Even with the crude methods of today's electro-encephalography, a person's brain tracings are as distinctive as his handwriting. So the resonance with his own past brain states would be of much greater strength than any other resonance.

(ii) Children are encouraged to learn to distinguish memories from dreams and phantasies, both by practical need and by parental training. Even so, no adult can fully distinguish perception, memory, and imagination. Most children get no such encouragement to learn to distinguish ESP from other aspects of psychological life.

Therefore it is not surprising to find memory more reliable than ESP.

9 *The Problem of Perception*

It is almost a truism that our perceptions may mislead us; but it sets psychologists a twofold task. They must find general laws governing the way we perceive things. And they must give these laws some neurophysiological basis. The vast array of theories of perception testifies to the difficulty of the task. Each theory fares better with some of the facts than with others. By means of the concept of resonance, it will be shown, we can construct a general theory of perception which combines the best features of several. Space permits only a brief discussion.

Phenomenologically, the Gestalt theory of perception has been highly successful.[1] Many experiments show that perception has a holistic character. Optical illusions or judgments of colour reveal how perception of parts of an object is influenced by the other parts. A melody sounds similar whatever the key: the structure of the whole is perceived more clearly than the individual notes. There are many such generalisations supported by experimental facts.

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[1] For a brief account see F. H. Allport, *Theories of Perception and the Concept of Structure*, New York and London, 1955, pp. 112-147

However, the Gestalt theorists have been less successful in finding a physiological basis for their holistic laws. They have suggested that brain function involves electric currents or fields in the cortex. (These macroscopic entities were thought holistic enough to explain both the Gestalt laws and Lashley's Law of Mass Action.) But there is evidence that electric fields and currents either do not exist in the cortex, or play no significant part in perception. For operations which would radically alter them (such as shielding the cortex with gold leaf) do not affect visual perception. We are left with the statement that parts of a percept are related as if by a field, but that no such field exists in the brain in terms of known physical entities.

Neurone theories of perception have a more solid physiological basis. Their difficulty is to account for the holistic qualities of perception. The most determined of them, the newer cybernetic theories,[1] postulate complicated scanning mechanisms. These can produce similar patterns of neural activity from, say, perceived squares of different sizes and orientations. They do not explain all the Gestalt phenomena. The more Gestalt-like the results that are to be explained, the more complex become the explanations. And these very complex and delicate mechanisms of perception must integrate with the memory mechanisms. Since memory mechanisms are not sensitive or localised, but obey the law of mass action, it is very hard to see how the two can successfully combine.

A neurone theory of perception which included resonance would go somewhat as follows. For the sake of clarity let us express it in terms of visual perception. The stimulation pattern on the retina is projected point for point on to the visual area of the cortex by the optic nerve. This is a relatively simple structure, so little resonance takes place. Patterns of ever greater complexity and interrelatedness are formed next in the visual association area, and then in the cortex in general. So more and more resonance takes place as the visual process becomes linked in the cortex in general with all other psychological functions. At an intermediate level, say in the visual association area, there will be a more equal proportion of retinal and resonance influences on the pattern. Let us suppose that it is the patterns in this area which correspond to visual perception, rather than the earlier or later ones.

It remains to show that the resonance effects will tend to make perception follow the Gestalt phenomenological laws. This cannot

[1] F. H. Allport, *Theories of Perception and the Concept of Structure*, New York and London, 1955, pp. 467-530

be done in detail here. But whatever could be explained by the Gestalt psychologists' idea of a cortical field, can be explained equally well by resonance. It has already been postulated (in section five of this paper) that resonances can be represented as attractions in an abstract pattern space. So a network of resonating structures will create a ' field ' of influences in pattern space. Here, seemingly, is a physical basis for field theories in psychology. In particular, the resonance concept unites the main trends in field and neurone theories of perception into one comprehensive whole.

10 *Summary*

Physical experiments are performed on simplified structures. There may be undiscovered holistic laws whose effects would be strong only in complex structures like the brain. Such a law is proposed, the ' law of resonance ', to explain the existence of telepathy and memory. By this law, any two similar structures exert an influence on each other which tends to make them become more similar. This influence can act on physical structures only within the limits of the indeterminacy principle, but is independent of their spatio-temporal separation. Resonance of a pattern of neural activity in a brain with another brain leads to telepathy; resonance with a past state of itself leads to memory. This theory fits the relevant facts in physics, neurophysiology, and psychology.

WHEN ARE STATEMENTS EMPIRICAL? *

J. W. N. Watkins

1 *Introduction*

Like all questions of classification, the question posed by the title of this paper has no intrinsic interest. To someone with no axe to grind, what matters about a statement is not whether to label it ' empirical ' or not, but what it says. But I have an axe to grind; and so have logical empiricists who have proposed answers to our question—though their axe is different.

They have contended that when someone stops making empirical statements he thereby stops making factual statements at all. For the logical empiricist, the correct answer to our question prescribes the limits of factual discourse. Our question therefore has the greatest extrinsic interest for him. I on the contrary have contended that when someone stops making empirical statements he may start making true-or-false statements of a factual but metaphysical character.[1] Thus our question also has great extrinsic interest for me: a correct answer to it would enable us to draw the line between science and metaphysics.

In my argument for the existence of genuine factual statements of a metaphysical character a crucial rôle was played by the contention that purely existential statements—bare assertions of existence—are properly regarded as unempirical, though factual. In support of this contention I offered some informal arguments and I also proposed a rough and ready criterion for ' empirical ' which excludes purely existential statements. But devising a criterion for ' empirical ' when one's aim is to exclude certain statements as unempirical is a tricky business. One must, of course, select a criterion which yields the desired answer; but this immediately exposes one to the charge of scoring a merely verbal success by a piece of *ad hoc* legislation.

I tried to forestall this charge by basing my criterion on one advanced by a leading logical empiricist—on the criterion proposed by Professor A. J. Ayer in the Introduction to the second edition of *Language*,

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* Received 20. iv. 59

[1] *Philosophy*, 1957, **32**, 112-131; *Mind*, 1958, **67**, 344-365

Truth and Logic. But this did not satisfy other logical empiricists.[1] I was told that my criterion is artificial and, moreover, that *any* criterion is bound to be artificial and inadequate because the notion of an empirical statement is inherently vague. Most chastening of all, I found that Ayer's criterion *does* allow purely existential statements to be empirical. Clearly, a fresh start was needed.

2 *Is An Adequate Criterion Impossible ?*

Professor Hempel, who has given much acute thought to our question, recently concluded: ' I have serious doubts as to the possibility of formulating a satisfactory general criterion for statements with empirical import.'[2] Now while this authoritative admission that the concentrated endeavours of logical empiricists over the last forty years to answer our question have yielded only the weary confession that the question is unanswerable—while this admission is disheartening to the logical empiricist who wants a sharp notion of ' empirical ' with which to determine what sentences are meaningful, it is also disheartening to an anti-empiricist like myself who wants to base his case on precise kinds of counter-example to the thesis that all factually meaningful statements are empirical. My anti-empiricist thesis is as much in need of a sharp and adequate criterion as was the old empiricist thesis.

Before we can decide whether an adequate criterion for ' empirical ' is indeed impossible we must state the requirements it should satisfy for it to be adequate. We can begin by laying down two formal requirements which no one would question:

(1) The criterion must not lead to inconsistent or paradoxical results.

(2) The criterion must exclude *some* sentences as lacking empirical import.

Next, we have to consider the more controversial question of material requirements. Now there are many kinds of statement about whose empirical or unempirical status there is doubt and controversy. For me, the most prominent case is that of purely existential statements; but there are other cases, such as singular probability-statements (about, say, the chances of a particular horse winning the Derby), comparisons between the intensities of different people's feelings, predictions about the appearance of the world after the human race is extinct, contrary-to-fact conditional historical statements (about, say, what would have happened if Cleopatra's nose had been shorter). On the other hand, there are a few kinds of statement about whose empirical or unempirical status there is widespread agreement. They may be regarded as fixed points, and the problem of finding an adequate criterion for ' empirical ' may be likened to the problem of finding a simple curve which passes through these points and extends to the disputed areas. The fixed points I have in mind are indicated by the following material requirements:

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[1] See, for example, Carl G. Hempel, ' Empirical Statements and Falsifiability ', *Philosophy*, **33**, 1958

[2] Ibid., p. 348

(3) (*a*) The criterion must include as empirical such statements as ' All planets move in ellipses ', that is, unrestricted experimental generalisations all of whose predicates refer to testable properties.

(*b*) The criterion must exclude manifest tautologies.

(*c*) The criterion must exclude those theological statements whose acceptability could never be affected one way or the other by observations. The bare assertion ' God exists ' is an example.

I admit that (3) (*a*) is not entirely uncontroversial, since there have been verificationists who have denied empirical status to experimental generalisations, preferring to regard them as schemata analogous to propositional functions, or else as rules of inference for deriving singular statements from singular statements. Clearly, these intrepid authors were trying to bend usage to their preconceived notions, rather than to discover a criterion which fits pre-existing usage. Now I am not in principle opposed to such linguistic reform, but in the present case there is a decisive objection to both these proposals: experimental generalisations do sometimes get experimentally falsified, but propositional functions, which are neither true or false, and rules of inference, are both unfalsifiable. The ' rule-of-inference ' interpretation tends to place scientific hypotheses beyond experimental disciplines. If we have the syllogism,

' All swans are white
This is a swan
Therefore this is white '

and the observation report,

' But this is black '

we are free to conclude that it is the universal major premiss, rather than the singular minor premiss, which is false. But if we re-write the major premiss as a rule of inference, so that we have,

Rule: Whiteness
is inferrable
from swanhood

' This is a swan,
Therefore this is white '

and the same falsifying observation report, then, since what was the minor premiss has now become the sole premiss, it looks as though we can conclude only *its* falsity from the observation report, since our ex-major premiss is now safely tucked away inside its box.[1]

As to (3) (*b*), I say ' manifest tautology ' (by which I mean logical truths like ' No unmarried man is married ' as opposed to sentences like ' No bachelor is married ') in the hope of keeping this requirement clear of the stormy debates which have raged over the analyticity of sentences which can be shown to be logical truths only by substituting synonyms for synonyms. I do not know of anyone who regards logical truths as empirical.[2]

I do not think that (3) (*c*) calls for comment.

One would have thought that it should not be so *very* difficult to find a criterion which satisfies these adequacy-requirements. Why does Hempel feel that there can be no adequate criterion? The short answer is that when Hempel surveyed the various criteria proposed by logical empiricists and by Popper (whose criterion Hempel mis-stated as a meaning-criterion) he found that all but a Carnapian translatability-criterion came to grief over our two *formal* requirements, and that the last raised serious difficulties of its own (in connection with theoretical and dispositional terms).[3] But it must be remembered that logical empiricists (unlike Popper) were not seeking a criterion simply for ' empirical '; they were seeking a single criterion for two quite different notions—' empirical ' on the one hand, and ' factually meaningful ' on the other. They wanted to kill two birds with one stone. Since my aim is to show that there are unempirical statements which are meaningful but not analytic, I obviously require a criterion for ' empirical ' which is *not* at the same time a criterion for ' factually meaningful '.

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[1] See the cogent criticisms of the ' rule-of-inference ' interpretation of scientific hypotheses and of principles of conduct in R. M. Hare, *The Language of Morals*, Oxford, 1952, pp. 45-55; see also J. O. Wisdom's review of *The Philosophy of Science* by Stephen Toulmin, *The Cambridge Journal*, 1954, **8**, 501-2, and H. Gavin Alexander's ' General Statements as Rules of Inference ' in Minnesota Studies in the Philosophy of Science, Vol. ii: *Concepts, Theories and the Mind-Body Problem*, ed. H. Feigl, M. Scriven, and G. Maxwell, Minneapolis, 1958.

[2] See, e.g. W. V. O. Quine, *From a Logical Point of View*, Cambridge (Mass.), 1953, pp. 22-23; Morton White, *Toward Reunion in Philosophy*, Cambridge (Mass.) 1956, pp. 127-132

[3] ' Problems and Changes in the Empiricist Criterion of Meaning ', in *Semantics and the Philosophy of Language*, ed. Leonard Linsky, 1952

Killing two birds with one stone is generally a very difficult thing to do, and I believe that in this case it is impossible. For in 1933 Popper showed that a realistic dividing-line between scientific and unscientific statements cannot coincide with a realistic dividing-line between meaningful and meaningless sentences, adding in 1934 that the bare negations of scientific statements, though equally meaningful, are untestable and unscientific.[1] For my part, I think that Hempel is right to despair of the possibility of an adequate *dual*-purpose criterion for ' empirical ' *and* ' factually meaningful '. But it may be that criteria which have proved inadequate as dual-purpose criteria will turn out to be adequate if interpreted as single-purpose criteria for ' empirical ' alone. The only way to show that Hempel is wrong to despair of the possibility of an adequate single-purpose criterion is actually to produce one which does satisfy the adequacy-requirements listed earlier, a criterion which distinguishes empirical from unempirical statements *within* the class of meaningful statements. This Popper has done.

But I have to do more than this. I have in mind an imaginary opponent of the view that purely existential statements should be regarded as unempirical. Hitherto he has inclined to the view that there cannot be an adequate criterion for ' empirical ' and hence that purely existential statements cannot be excluded as unempirical by any adequate criterion. But now he has swung over to the view that there are no doubt several tolerably adequate criteria, some of which do, and some of which do not, exclude purely existential statements. ' Pick the appropriate criterion and you'll get the result you want ', he might say, ' but that won't stop other people operating with other criteria according to which purely existential statements *are* empirical '.

For me, the ideal answer to this would be, first to prove that *any* criterion which classifies purely existential statements as empirical will fail to satisfy one or more of the adequacy-requirements listed earlier, and secondly to produce a criterion which does satisfy these requirements and which classifies purely existential statements as unempirical. This answer, if it could be given, would completely rebut all charges of *ad hoc* terminological legislation.

I cannot, however, prove in advance the inadequacy of every unheard-of criterion which might classify purely existential statements as empirical. I can only offer a second-best answer to the complaint of my imaginary critic. This answer will consist of establishing the following two theses. First, every type of criterion *so far proposed* which classifies purely existential statements as empirial is inadequate. Second, there is a criterion—viz. Popper's—which is adequate and which classifies purely existential statements as unempirical—' adequacy' being defined in terms of the adequacy-requirements laid down earlier.

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[1] Karl R. Popper, *The Logic of Scientific Discovery*, London, 1959, App. 1 and Sect. 15

3 *Existing Types of Criterion*

I do not want to add to the literature on the history of verificationist and other theories of empirical meaningfulness more than is absolutely necessary to allow us to see what different types of criterion have emerged. It seems to me that the various types of meaning-criterion which have been proposed can be interpreted as more modest substitutes for the over-ambitious doctrines in Wittgenstein's *Tractatus*.[1] Wittgenstein did not *propose* a verificationist criterion for meaningfulness. He claimed that one can *demonstrate* the meaninglessness of philosophical and metaphysical ' propositions ' (6.53). It is not that they turn out to be meaningless according to some suggested criterion—they are *inherently* meaningless.[2] ' Most questions and propositions of the philosophers result from the fact that we do not understand the logic of our language ' (4.003). Their apparent meaningfulness and real senselessness result from the fact that although they *appear* to be grammatical they *really* contravene the rules of logical grammar (4.003). Now these rules are not conventional: ' the rules of logical syntax *must follow of themselves*, if we only know how every single sign signifies ' (3.334, my italics). Wittgenstein apparently regarded it as a part of, or a consequence of, the logical syntax of a language that all genuine propositions must be 'results of truth-operations on the elementary propositions' (5.3), an elementary proposition being a true or false assertion of the existence of an atomic fact. If elementary propositions may be established by observation, as presumably they may, it follows that, according to Wittgenstein, every meaningful proposition is both verifiable if true and falsifiable if false. Thus for Wittgenstein, verificationism is inherent in the logical structure of language. I think that the key to his verificationism is to be found in proposition 4.26 of the *Tractatus:*

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[1] Popper's theory of demarcation, on the other hand, was always in flat opposition to Wittgenstein's doctrines. See K. R. Popper, ' Philosophy of Science: A Personal Report ' in *British Philosophy in Mid-Century*, ed. C. A. Mace, London, 1956, esp. pp. 163-164.

[2] In other words, Wittgenstein held what Popper calls the naturalistic theory of meaninglessness: ' By the naturalistic theory of meaninglessness I mean the doctrine that every linguistic expression purporting to be an assertion is either meaningful or meaningless; not by convention or as a result of rules which have been laid down by convention, but as a matter of actual fact, or due to its nature, just as a plant is, or is not, green in fact, or by nature, and not by conventional rules.' Karl R. Popper, ' The Demarcation between Science and Metaphysics ', contribution to the planned Carnap volume of the *Library of Living Philosophers*, ed. P. A. Schilp, mimeograph, p. 10. My understanding of the ideas of the Vienna Circle is very much indebted to this paper, and to conversations with its author.

> The world is completely described by the specification of all elementary propositions plus the specification, which of them are true and which false.

From this proposition (which mirrors the earlier metaphysical proposition 2.04: 'the totality of existent atomic facts is the world') it follows that a 'proposition' which is not a truth-function of elementary propositions *cannot* be a part of the description of the world and *must* therefore be a pseudo-proposition. This view I consider quite false; for a list of all true elementary propositions would not describe the *structure* of the world, the invariant relations *between* singular facts. These require for their description scientific laws. But scientific laws cannot be truth-functions of elementary propositions. Already in the *Tractatus* we find something of that disparaging attitude towards scientific laws which was to become a characteristic feature of verificationistic literature. Wittgenstein appears to have held that 'the so-called laws of nature' do not enable us either to explain or even to predict natural phenomena.[1]

Few logical empiricists were much perturbed by this unintentional exclusion of scientific hypotheses. What inspired them to renewed efforts was the realisation that the logical grammar inherent in a natural language is not sufficiently rigorous and tough to exclude metaphysics unaided: it requires artificial reinforcing if metaphysics is to be finally silenced. There appeared to be two main alternative ways of achieving this.

The first was to construct an artificial language with a predetermined syntax and vocabulary such that every empirical assertion, and no unempirical non-tautological assertion, could be formulated within it. This idea was already in the *Tractatus*. For although Wittgenstein said that 'All propositions of our colloquial language are actually, just as they are, logically completely in order' (5.5563), he also said that a colloquial language largely disguises its logical grammar (4.002), thereby making possible those unwitting grammatical confusions 'of which the whole of philosophy is full'. He goes on: 'In order to avoid these errors, we must employ a symbolism which excludes them' (3.325). Thus one way by which it was hoped that metaphysics could be artificially excluded was by constructing what might be called an indubitably empirical *language* and declaring that no assertion is factually meaningful unless it can be expressed by a well-formed sentence within this language.

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[1] See *Tractatus* 6.371 and 5.134-5.1362. It was primarily against this disparaging attitude to scientific hypotheses, together with Wittgenstein's implicit admission that unscientific nonsense may be deeply significant, that Popper protested. See K. R. Popper, *The Open Society and its Enemies*, London, 1952, Vol. ii, pp. 296-299.

The other main way of excluding metaphysics did not require the construction of an artificial language with a manifest logical syntax and restricted vocabulary. It consisted of characterising a favoured class of what might be called indubitably empirical *statements* and then, instead of saying with Wittgenstein that all other genuine statements just *are* as a matter of grammatical fact truth-functions of these, *stipulating* that other statements are not to be classified as factually meaningful unless they stand in certain required logical relationships to some indubitably empirical statements.

At first, this latter type of criterion was given a verificationist character. A statement was allowed to be factually meaningful only if it was entailed, or rendered more probable, by indubitably empirical statements. But as a result of Popper's emphasis on the rôle of tests and refutations in science, there was a tendency to reverse the direction of the entailments and to replace a verifiability by a falsifiability criterion which requires of a factually meaningful statement that (perhaps in conjunction with certain kinds of subsidiary premisses) it entails, or renders more probable, one or more indubitably empirical statements (not entailed or rendered probable by the subsidiary premisses alone). Ayer's criterion was, I think, intended to have this character. Finally, there were logical empiricists who preferred to play safe by constructing a criterion out of the disjunction of a verifiability and a falsifiability criterion. For them, a statement was factually meaningful if it is either verifiable or falsifiable in the above senses.

Thus the following main types of criterion have so far been proposed:

(1) Translatability criteria;
(2) Verifiability criteria;

(3) Verifiability-*or*-Falsifiability criteria;
(4) Falsifiability criteria.

Although these were mostly proposed as criteria for factual meaningfulness, I am mainly concerned with their adequacy or inadequacy as criteria for 'empirical' and with their bearing on the question of the empirical or unempirical status of purely existential statements. The first three types of criterion all classify bare assertions of the existence of an observable object as empirical. Whether or not a falsifiability criterion classifies such purely existential statements as empirical will be found to depend on the precise status of the subsidiary premisses with which it allows a statement to be combined when under examination for empirical content. Thus the fourth type of criterion sub-divides into:

(4) (*a*) Falsifiability criteria which classify purely existential statements of a verifiable character as empirical;
(*b*) Falsifiability criteria which classify all purely existential statements as unempirical.

My first thesis is, then, that criteria for 'empirical' of types (1), (2), (3) and (4) (*a*) are all inadequate, and my second thesis is that a criterion of type (4) (*b*) is adequate in terms of the requirements listed earlier. I will consider these types of criterion in the above order. I have nothing to add to criticisms which have already been levelled against the first three types, and my discussion of their defects will be brief. The bulk of this paper will consist of a comparison between (4) (*a*) and (4) (*b*).

(1) *Translatability Criteria.* Advocates of this type of criterion admit that there are difficulties in building up from observation-predicates a vocabulary containing dispositional and theoretical terms.[1] But there is no need to enter the intricate argument over the possibility of bridging the gap between abstract terms (e.g. 'electromagnetic field') and observation predicates (e.g. 'solid') by means of Reichenbach's 'co-ordinating definitions', Carnap's 'reduction-sentences', etc. For the fact is that translatability-criteria violate our adequacy-requirement (3) (*c*)—they allow the formulation of sentences expressing transcendent theological assertions. Thus Popper has translated what he calls the arch-metaphysical sentence, viz. 'There exists an omnipotent, omnipresent and omniscient personal spirit',

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[1] See Hempel, ibid., and I. Scheffler, 'Prospects of a Modest Empiricism', *Review of Metaphysics*, 1957, **10**, 383-400 and 602-625

into a well-formed formula in a language similar to Carnap's language for unified science.[1] Popper's ability to construct such a counter-example was not due to some accidental deficiency in the language in question. So long as the purified empirical language contains ordinary empirical predicates, the existential quantifier *and the negation sign*, it will always be possible to formulate within it theological assertions such as ' There exists an x such that x was *not* born, will *not* die, is *not* divisible into parts, is *not* bounded, is *not* material; and there exists *no* y (where $y \neq x$) such that x is ignorant of y or such that x cannot alter y or such that y can alter x '. (The phrase after the semi-colon guarantees the uniqueness of x.) It is worth recalling that Descartes, for instance, arrived at his conception of the Perfect Being from a consideration of his own limitations (*Discourse*, pt. iv).

(2) *Verifiability criteria.* Such criteria violate our adequacy-requirement (3) (*a*)—as is well known, they rule out unrestricted experimental generalisations. No finite number of singular observation statements can raise the probability of a universal law above zero (let alone raise it to 1, i.e. verify it) because such a law asserts something about infinitely more instances than are mentioned by the observation statements. And if, in order to avoid the attribution of the same zero-probability to all universal laws (whether corroborated, falsified, or self-contradictory), the empirical confirmation of a universal law is made a function of the ratio between the number of favourable instances and the *finite* number of *observed* instances (as opposed to the *infinite* number of *possible* instances) then, as Popper has pointed out,[2] different but equally absurd results, will ensue—for instance, a hypothesis which has been decisively falsified may nevertheless be graded ' highly probable ' or ' highly confirmed '.

(3) *Verifiability-or-Falsifiability Criteria.* Double-barrelled criteria of this type do not exclude experimental generalisations but, as is also the case with verifiability criteria, they violate our adequacy-requirement (3) (*b*)—they include manifest tautologies as empirical. An observation-statement trails behind it processions of statements of diminishing content, each entailed by, but not entailing, each of its predecessors in the procession. Near the rear of such processions come vacuous existential statements such as ' There exists something which is white or non-white ', while at the rear come all tautologies. The verificationist

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[1] Popper, ' The Demarcation between Science and Metaphysics ', pp. 34-36

[2] Ibid., pp. 42-44. See also *The Logic of Scientific Discovery*, sect. 80 and App. ix

half of disjunctive criteria requires all such vacuous statements to be counted as empirical. But I think most philosophers would agree with Wittgenstein when he said: ' Not only must a proposition of logic be incapable of being contradicted by any possible experience, but it must also be incapable of being confirmed by any such ' (*Tractatus*, 6.1222).

(4) *Two Types of Falsifiability Criteria.* Having hurriedly disposed of the first three types of criterion I shall devote most of the remainder of this paper to a comparison between the two types of falsifiability criteria. First, however, I want to be rather more explicit about what I have called ' indubitably empirical statements ', or what Popper calls ' basic statements '.

Basic statements provide a touchstone for the empirical sciences, for historical reconstructions, and indeed for all our empirical beliefs about the public, four-dimensional world. Two things therefore tend to be demanded of them. (*a*) Since they are a touchstone for *synthetic* hypotheses about the *public* world, they too must be synthetic assertions about public events (or things, or states of affairs). (*b*) Since they provide a *touchstone*, they should, if accepted, be indisputable. As is well known, however, (*a*) and (*b*) cannot both be satisfied. The man who, confronted by a black object, says ' That is a raven ', makes a synthetic assertion which is not indisputable, whereas the man who says 'I am convinced that that is a raven ', or ' That seems to me to be a raven ', or ' I call that a raven ', may say something indisputable; if so, his assertion is about himself rather than the black object, for being indisputable it would not be refuted if the black object turned out to be no raven.

Popper overcame this difficulty by regarding a basic statement as a true-or-false singular existential statement about some easily observable state of affairs, a statement which we can *test to our full satisfaction.*[1] ' There is a black swan in my study today ' is a basic statement, but ' There is a black swan in Africa ' and ' There is a grain of pepper in my study today ' are not basic statements. An accepted basic statement is one we have *decided* to accept *because* we are, for the time being, satisfied by the outcome of our tests of it. But strictly speaking, tests cannot *verify* a basic statement. There is always the logical possibility, at any rate, that the outcome of subsequent tests (provoked, perhaps, by the publication of a new theory) will cause us to withdraw our earlier acceptance of it. Accepted basic statements of this kind will satisfy demand (*a*) above, and also a revised demand (b^1) namely, that while not logically indisputable they may be tested to our full satisfaction, and provisionally accepted as if verified. This means that a basic statement cannot contain any metaphysical ingredient. 'There is a black swan with an immortal soul in my study today', is testable, but it could not be accepted as tested to our full satisfaction. This point will prove important later.

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[1] *The Logic of Scientific Discovery*, ch. 5

If it is agreed that the class of indubitably empirical statements is to consist of basic statements as characterised above, the question now arises: what logical relations, according to a falsifiability type of criterion, must an empirical statement outside this class have to statements inside it?

Two answers have been given to this question. The difference between them appears at first sight to be small and technical. But like the horseshoe nail for want of which a kingdom was lost, it will turn out to have the largest consequences.

For Popper, a statement, or system of statements, is empirical if and only if it contradicts at least one true-or-false basic statement. Such a basic statement he calls a 'potential falsifier' of the empirical statement or system. (If a potential falsifier is accepted as true it becomes an actual falsifier.) If the empirical statement is a universal hypothesis, its potential falsifiers will usually be composite basic statements. Suppose the hypothesis h in conjunction with the basic statement b_1 entails a further basic statement b_2. (For brevity, I say 'entails a further basic statement' instead of 'entails a basic statement not entailed by b_1 alone'.) Then the composite basic statement b_1 *& not*-b_2 is a potential falsifier of h.

The empirical content of h is measured by the relative size of the class of its potential falsifiers.[1]

The point to which I especially wish to draw attention is this. Popper's requirement that a universal hypothesis h is empirical if and only if it is contradicted by some basic statement is equivalent to the requirement that h is empirical if and only if, in conjunction with *subsidiary premisses consisting of nothing but basic statements* it entails further basic statements.

On the other hand, Ayer allows h to be empirical (or 'indirectly verifiable') if it entails further basic (or 'observation') statements when combined with *subsidiary premisses consisting of basic statement and/or other statements independently established as empirical* (whether as 'directly verifiable' or 'indirectly verifiable').[2]

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[1] *The Logic of Scientific Discovery*, ch. 6

[2] A. J. Ayer, *Language, Truth and Logic*, London, 1946, p. 13

Ayer's recursive approach seems, at first sight, intuitively acceptable, especially if one bears in mind the hierarchical, or pyramidical, structure of the theoretical systems of science. According to it, we are to proceed step-by-step in the award of certificates of empirical status. We start with an initial class of indubitably empirical statements (basic or observation statements). Subsequently, whenever a statement not included in this initial class is found to entail further indubitably empirical statements when combined with indubitably empirical subsidiary premisses, then it joins the class of indubitably empirical statements and may itself be employed as a subsidiary premiss when subsequent statements are examined for empirical status.

By allowing low-level, 'directly verifiable' generalisations, or experimental laws, to mediate between basic statements and high-level, 'indirectly verifiable' abstract theories, or 'Campbellian hypotheses' as they are sometimes called, which refer only to unobservable entities and properties, this recursive procedure appears to handle such abstract theories in an intuitively more satisfactory way than does Popper's criterion, which allows only basic statements to be employed as subsidiary premisses when a theoretical system is being examined for empirical status. This may be indicated by an example which I will keep as simple as possible. Suppose we have a theory t_1:

$$(t_1) \qquad c \supset \epsilon \;\&\; \epsilon \supset d$$

where the roman letters 'c' and 'd' each stand for an increase in some directly testable magnitude (say, in the temperature of a gas in a container and in the pressure in the container), and where the Greek letter 'ϵ' stands for an increase in some unobservable magnitude (say, the average speed of the gas-molecules in the container). And suppose we have a second theory of a similar character:

$$(t_2) \qquad e \supset \theta \;\&\; \theta \supset f$$

where e and f stand for decreases in observable magnitudes (volume and work) and θ for an increase in an unobservable magnitude (internal energy of the gas).

t_1 and t_2 are empirical for both Popper and Ayer. Now suppose we have an abstract theory, or Campbellian hypothesis (increase of gas energy implies increase of speed of molecules), which mentions only the unobservable magnitudes mentioned by t_1 and t_2:

$$(t_3) \qquad \epsilon \supset \theta$$

By itself, t_3 is unempirical for Popper, since no basic statement can contradict it; but not for Ayer. Let b_1 report an instance of c and

b_2 predict an instance of *f*. Now b_2 can be deduced from t_3 in conjunction with t_1 & t_2 & b_1 (already established as empirical) but not from t_1 & t_2 & b_1 alone. Therefore t_3 is empirical for Ayer.

By Popper's criterion the whole theoretical system t_1 & t_2 & t_3 is empirical (b_1 & *not*-b_2 is a potential falsifier of it); but t_3 considered in isolation is a free-floating metaphysical assertion about unobservable entities. It becomes part of an empirical system only when it is moored to the empirical basis with the help of intermediate, lower level theories.

Ayer's recursive procedure allows us to regard an abstract statement in the upper reaches of a scientific system as empirical when considered in isolation because this procedure allows empirical statements other than basic statements to be employed as subsidiary premisses when the abstract statement in question is being examined for empirical status. Instead of examining a hierarchical system of statements for empirical status *en bloc*, it allows a statement-by-statement examination. We work methodically upwards from the basic statement level; and at each stage the statement under consideration proves its empirical status if its inclusion within a set of statements already established as empirical enhances their predictive power.

I will now compare the implications of Ayer's and Popper's criteria of 'empirical' for my main argument, published elsewhere, about metaphysical statements, an argument which, as I have said, hinges on the contention that purely existential statements are properly regarded as unempirical.

Ayer calls his criterion a 'verification-principle': it classifies meaningful non-analytic statements into 'directly verifiable' and 'indirectly verifiable' statements. This nomenclature strikes me as unfortunate. It seems natural, first to describe a universal statement like 'All swans are white' as unverifiable but falsifiable (because no finite number of observation statements could verify it, though one observation statement could falsify it); and secondly, to describe a purely existential statement like 'Black swans exist' as verifiable but unfalsifiable (for complementary reasons). Yet Ayer's criterion classifies the *un*verifiable 'All swans are white' as 'directly verifiable'; on the other hand, it does *not* classify the verifiable 'Black swans exist' as 'directly verifiable'. The fact is that Ayer's criterion is a falsifiability rather than a verifiability criterion in the sense that it requires an empirical hypothesis to entail rather than to be entailed by, observation statements whose falsity would falsify, but whose truth would not verify the hypothesis. It was these considerations which encouraged me to rely on it when I first advanced my anti-empiricist thesis. I supposed that by this criterion purely existential statements are not even 'indirectly verifiable'. But I subsequently discovered that in this I was mistaken.

Consider the following statements:

(*b*) There is a black swan in my study today.
(*h*) There is no black swan outside my study.
(*s*) A black swan exists.

For both Ayer and Popper, *b* is an observation or basic statement and *h* is a falsifiable hypothesis. Now for Popper, *s* by itself is unempirical because there is no basic statement, however complex, which could contradict it—it has no potential falsifiers. But for Ayer, *s* is 'indirectly verifiable' because in conjunction with the 'directly verifiable' *h* it entails *b* which is not entailed by *h* alone.

Thus the answer to what at first sight seemed a small technical question—may the subsidiary premisses employed when a statement, or system of statements, is being examined for empirical status include empirical statements other than basic statements?—the answer to this question turns out to be crucial for my main argument about metaphysical statements. If the answer is 'Yes', that argument collapses. Moreover, this answer appears to be plausible in itself and prompted by the desirability of accommodating those abstract Campbellian hypotheses (such as our imaginary t_3) which do not entail any further basic statements when combined only with basic statements.

But in fact the answer has to be 'No'. We have no choice here. For the decisive fact is that *a falsifiability-criterion which allows empirical statements other than basic statements to figure among the subsidiary premisses employed when a statement is being examined for empirical status will allow any statement whatever to be 'empirical'*.

The reason is this. If we tack a metaphysical statement *m* on to a basic statement *b*, the resulting statement *b & m* (e.g. 'There is a black swan with an immortal soul in my study today') will have as much empirical content as *b*. But unlike *b*, *b & m* could never be accepted as tested to our full satisfaction. It is therefore not a basic statement. Now if we tack *m* on to a universal empirical hypothesis, *h*, the resulting statement *h & m* has just as much predictive power and just as many potential falsifiers as *h*. *h & m* cannot, of course, be tested to our full satisfaction. But neither can *h*, for *h* is a universal hypothesis. Thus from the testability point of view, *b* and *b & m* are very different (one might but the other could not be accepted as tested to our full satisfaction); but *h* and *h & m* are on a par (both can be tested but neither could be accepted as tested to our full satisfaction). In other words, whereas to establish that a statement is a basic statement *is* to establish that it has no metaphysical component, to establish that a statement is an empirical hypothesis, to establish that it has predictive power and that it is experimentally falsifiable, is *not* to establish that it has no metaphysical component. Consequently, if we allow empirical hypotheses, as opposed to basic statements, to figure among the subsidiary premisses employed when a statement *s* is being examined for empirical status, we cannot ensure that there are no metaphysical components lurking among the subsidiary premisses. Thus if *s* were nothing but a piece of unmitigated metaphysics it might nevertheless engage, so to speak, with the metaphysical components within the empirical subsidiary premisses and thereby indirectly generate a basic statement. For example, we can employ Ayer's recursive procedure to establish that *s*, ' The Absolute is lazy ', is empirical, as follows. The hypothesis *h*, ' All swans are white and the Absolute is not lazy *or* This is red ', is ' directly verifiable ' because, in conjunction with b_1, ' This is not red and is a swan ', it entails b_2, ' This is white '. Now *s* will be ' indirectly verifiable ' if, in conjunction with *h*, it entails an observation statement not entailed by *h* alone. And this is the case. For *h* does not, whereas *s & h* does, entail b_3, ' This is red '.[1]

That a falsifiability criterion which allows empirical hypotheses other than basic statements to be employed as subsidiary premisses is bound to let everything through can be shown more simply by reverting to our imaginary theory t_1.[2] From the fact that ' $c \supset \epsilon \,\&\, \epsilon \supset d$ ' is testable, or ' directly verifiable ', it by no means follows that it has no metaphysical ingredient. Instead of standing for an increase in some scientifically respectable though unobservable magnitude, such as the average speed of the gas-molecules in a container, ' ϵ ' might stand for an increase in the laziness of the Absolute; in which case *s*, ' The Absolute has grown lazier ' would be ' indirectly verifiable ', since in conjunction with the 'directly verifiable' t_1 it would entail the prediction b_1, 'the pressure in the container has increased', and b_1 is not entailed by t_1 alone.

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[1] Professor A. Church was the first to show that Ayer's criterion lets all sentences through. His example was this. Let O_1, O_2, O_3 be observation sentences, and *s* be any sentence whatever. Let *h* be the sentence ' (*not*-O_1 & O_2) or (O_3 & *not-s*) '. *h* is ' directly verifiable ', since in conjunction with O_1 it entails O_3; and since *s & h* entails O_2, *s* is ' indirectly verifiable '. *Journal of Symbolic Logic*, 1949, **50**, 14.

[2] As Popper pointed out when I read an earlier draft of this paper at his Seminar at The London School of Economics.

Thus Ayer's version of the falsifiability criterion violates my second adequacy-requirement.

This completes my argument in support of my first thesis, namely, that all types of criterion for 'empirical' so far proposed which classify purely existential statements as empirical, fail to satisfy the adequacy-requirements mentioned earlier. I now turn to my second thesis, namely, that there is a criterion—viz. Popper's version of the falsifiability criterion—which excludes purely existential statements as unempirical and which does satisfy those requirements.

Popper's criterion for 'empirical' obviously satisfies the second and the whole of the third adequacy-requirement. But Hempel has suggested that this falsifiability-criterion has paradoxical consequences —in other words, that it violates our first adequacy-requirement. True, Hempel states it as a meaning-criterion;[1] but we must consider the possibility that his objections may also apply to it when advanced simply as a criterion for 'empirical'.

Hempel showed that both verifiability and falsifiability criteria for factual meaningfulness are hit by what might be called (*a*) the 'asymmetry-paradox' and (*b*) the 'tacking-paradox'.

(*a*) The asymmetry-paradox results from the fact that, although the negation of a meaningful statement should presumably be equally meaningful, these two meaning-criteria give asymmetrical results for universal statements and their purely existential denials.

Popper's criterion does, of course, give asymmetrical results: it says that the bare negation of a scientific hypothesis, though equally meaningful, is not a scientific hypothesis. But there is nothing paradoxical about this. On the contrary: to insist that the negation of a statement must not only be equally meaningful but must, moreover, have the same empirical or scientific status as that of the statement it negates would have the strange consequence that science would mainly consist of a body of verified truths. If the bare negation of a scientific hypothesis were itself a scientific hypothesis, the falsification of one would mean the verification of the other. When one looks into the writings of those philosophers of science who, like Bacon,[2] have believed that a crucial experiment, by overthrowing one hypothesis, *does* establish a rival hypothesis, one invariably finds that they were treating what were actually *contrary* universal hypotheses (e.g. ' All light travels instantaneously ' and ' All light travels at a finite speed ') as if they were *exhaustive* alternatives, i.e. contradictory hypotheses.

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[1] Remarking in a footnote, 'Whether Popper would subscribe to the proposed restatement of the falsifiability criterion, I do not know' (ibid., p. 170); but Popper had been resisting this interpretation since 1933.

[2] See e.g. *Novum Organum*, ii, 36.

The prejudice that a sentence and its negation must share the same status can be dispelled by a consideration of commands, whose negations are not usually commands. If an Admiral signals ' No shore leave is to be granted today ', and next signals, ' Negative my previous signal ', his second signal is not, of course, a command that shore leave be granted today. Or consider prohibitions. The negation of ' Smoking is prohibited ' is not, of course, ' Non-smoking is prohibited '. The asymmetry between prohibitions and their unprohibiting negations is closely analogous to the asymmetry between scientific theories and their merely existential negations, for laws of nature may be regarded as prohibiting various logically possible states of affairs.[1]

Thus Popper's criterion is not hit by the asymmetry-paradox. It yields asymmetrical results which are not paradoxical. It is not a meaning-criterion, so it never classifies one statement as meaningful and its negation as meaningless.

(*b*) We must now consider whether it is hit by the tacking-paradox. This paradox arises in the following way. It is an old empiricist idea that a molecular sentence cannot be meaningful unless all of its constituents are meaningful (and also properly put together). Suppose we have a molecular sentence composed of two sub-sentences, one meaningful and the other meaningless according to some meaning-criterion. Then the molecular sentence as a whole will be *meaningless*. Suppose, secondly, that this criterion declares a sentence meaningful if it stands in a certain logical relationship to a meaningful sentence, and also that our molecular sentence stands in this relationship to the meaningful sub-sentence. Then the molecular sentence will be *meaningful*. Thus according to a verifiability criterion, if h is verifiable and m is metaphysical, then *h or m* as a whole is meaningless (it has a meaningless constituent) and also meaningful (it is verifiable by h). Similarly, if h is falsifiable, h & m is both meaningless and meaningful on a falsifiability criterion of meaning, and both *h or m* and h & m are both meaningless and meaningful on a verifiability-*or*-falsifiability criterion.

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[1] ' Not for nothing do we call the laws of nature " laws ": the more they prohibit the more they say.' (Popper, *The Logic of Scientific Discovery*, p. 41.)

Now in discussing tacking in connection with logical empiricists' meaning-criteria it is in order to employ '*m*' to stand indifferently for both metaphysical statements and meaningless strings of words, since their purpose was to class the former among the latter. But the purpose of Popper's criterion was to segregate empirical from other statements *within* the class of meaningful statements. Thus the critic who tries out the tacking gambit against it must specify whether it is some nonsensical string of words, like 'Saturday is in bed', which is to be tacked on to a falsifiable hypothesis, or some meaningful metaphysical statement, like 'Every event has a cause'.[1] In neither case will any paradox result.

Take the first alternative. Let us accept, for the sake of argument, the empiricist view that a sentence as a whole is meaningless if any of its constituents are meaningless; then if we tack 'Saturday is in bed' on to 'All ravens are black' the whole sentence which results does *not*, on Popper's criterion, express an *empirical* statement because it does not express a *statement*. His criterion distinguishes empirical from other kinds of genuine statement, not statements from pseudo-statements.

Now take the second alternative. This is less trivial, because in the history of science it has often happened that a powerful empirical theory has later been found to have metaphysical ingredients concealed within it, to have an '*h & m*'-character. As we have seen, Popper's criterion classifies such falsifiable though partially metaphysical theories as empirical. Now this classification would be paradoxical only if the addition of a metaphysical statement to an empirical statement renders the whole resulting statement metaphysical. But this is obviously not the case. When Ernst Mach complained that within Newton's theory there is 'an idle metaphysical conception' (namely, Newton's conception of absolute time) [2] he certainly was not complaining that Newton's whole theory was thereby idle and metaphysical.

'Ah', you may say, 'but in Newton's case the *h*-component was large and the *m*-component small. Metaphysics, like arsenic, may be harmless in small doses. But what about large doses? Suppose a metaphysician wished to write a tome full of the higher nonsense: to get past Popper's criterion would it not be enough for him, first to declare in his preface that his ideas have an organic unity and that his book consists of a single, unbroken statement, and second to begin chapter one with some well confirmed though falsifiable statement followed by " and " and the rest of the book? '

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[1] I have shown elsewhere that this metaphysical doctrine is factually true-or-false (and therefore meaningful). See *Mind*, 1958, **67**, esp. pp. 348-349.

[2] Mach, *The Science of Mechanics*, 5th English edition, Chicago, 1942, p. 274

Let us assume that this imaginary tome does at least make intelligible statements and does not contain nonsensical strings of words. Now Popper's falsifiability-criterion does not merely award certificates of empirical status to candidates who pass; it also *grades* them according to its measure of their empirical content. It would give this metaphysician's tome a miserably low grading; but if a statement, however verbose, has some empirical content, however meagre, it has to be classified as empirical. What this tome says would be empirical in the same way that a large field with a solitary cauliflower planted in it would be a cultivated field: it is not entirely uncultivated, nor is the tome entirely unempirical.

Thus Popper's criterion avoids the tacking-paradox.

I want now to consider a consequence of his criterion which, while it does not conflict with any of my adequacy-requirements, may seem intuitively unsatisfactory to some people. I am referring to the consequence already noted, that a theoretical system t (comprising, say, the theories t_1, t_2, and t_3 mentioned previously) may be empirical as a whole, while some of its components (t_3 in our example) are, considered in isolation, unempirical.

Now even if this consequence were genuinely unsatisfactory, our experience of what happens when statements other than basic statements are permitted as subsidiary premisses when a hypothesis is being examined for empirical status—our experience here suggests that the cost of avoiding this consequence is likely to be prohibitive.

But in fact I do not think that this consequence is unsatisfactory. Obviously, being a component of an empirical system cannot *entail* that the component on its own is empirical—it may be a tautology or a piece of mathematics. And once freed from the analytic/empirical dichotomy and the tiresome insistence that every intelligible unit of discourse, however fragmentary, which is not actually analytic must therefore be empirical, there is no reason to deny that a component pulled out from an empirical system may not, as in the case of our t_3, be a factual statement which, being no longer connected to the empirical basis, is by itself unempirical. This view is much less far-reaching than a view which has become popular, according to which only the *entire* theoretical structure involved in any scientific prediction or experiment is empirical, *all* subsidiary systems being separately unempirical. This whole-hogging view originated, so far as I know, with Duhem, when he pointed out that the falsification of a prediction does not tell us which part of the theoretical structure is to blame: 'If the predicted phenomenon is not produced, not only is the proposition questioned at fault, but so is the whole theoretical scaffolding used by the physicist.'[1] Professor W. V. O. Quine has relied on this idea in an attempt to show that the empiricist cannot segregate analytic from synthetic statements by defining the former as those which defy overthrow by all possible experiences. 'Statements are tied to the testimony of the senses only in a systematic or holistic way which defies any statement-by-statement distribution of sensory certificates.'[2] 'Our statements about the external world face the tribunal of sense experience not individually but only as a corporate body. . . . The unit of empirical significance is the whole of science.'[3]

Popper's criterion involves no such thorough-going holism as this. Within the entire structure of ideas explicitly and implicitly involved in a scientific prediction it will usually be possible to distinguish various separately testable, and therefore individually empirical, sub-systems. Only if a component is too fragmentary for it to be testable on its own is it unempirical. The separate testability of sub-systems also means that a falsified prediction need not drive scientists to the despairing conclusion that the entire structure of science is shaky (though this might conceivably be true). They can go on to investigate *which* sub-systems are faulty.

4 *Conclusion and Summary*

Four main types of criterion have been proposed. These are, in the order in which I have discussed them:

(1) Translatability criteria;
(2) Verifiability criteria;
(3) Verifiability-*or*-falsifiability criteria.
(4) Falsifiability-criteria, which sub-divide into:
  (*a*) those allowing falsifiable non-basic statements among the subsidiary premisses,
  (*b*) those allowing only basic statements among the subsidiary premisses.

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[1] P. Duhem, *The Aim and Structure of Physical Theory*, Princeton, 1954, p. 185

[2] W. V. O. Quine, *Mind*, 1953, **62**, 434

[3] Quine, *From a Logical Point of View*, pp. 41-42

All these types of criteria fail if advanced as dual purpose criteria for both ' empirical ' and ' factually meaningful'.[1] But if advanced as single-purpose criteria for ' empirical ', then (1), (2), (3) and (4) (*a*), which all classify many purely existential statements as empirical, all fail to satisfy one or more of the adequacy-requirements. (4) (*b*) alone meets these requirements and it alone excludes purely existential statements as unempirical. I am tempted to induce from these findings the conjecture that *any* criterion which includes any purely existential statements as ' empirical ' will come to grief; for no purely existential statement makes any observable difference; and once you start including statements which make no observable difference as ' empirical ' there is no telling what will happen. Still, this is only a conjecture, plausible as it seems to me. In the meantime, it seems clear that although (4) (*b*) is by no means *le dernier cri* by current fashions, it is still the best criterion on the market today. And according to this criterion I made no mistake when I took purely existential statements as the starting-point for a theory of metaphysics.

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[1] I fully agree with J. L. Evans, when, towards the end of his excellent article ' On Meaning and Verification ', he speaks of ' the questionable assumption that empirical sentences exhaust the category of sentences which are used to make true-or-false assertions ' and adds: ' To say of a given sentence that it can be verified is not to say anything about the meaningfulness of the sentence . . . ', *Mind*, 1953, **62,** 16.

DISCUSSIONS

New Argument for Induction: Reply to Professor Popper

It would not be in accordance with tradition for me to seek your space to reply to Dr Bronowski's interesting review of my *Foundations of Inductive Logic;* but I believe that such an inhibition need not apply to my commenting on Professor Popper's supplementary note. My rejoinder will be divided into three parts. The first part will be concerned with what Professor Popper calls the 'first contract', the second with the 'second contract' and the third with his analogy from telegraph poles. Professor Popper regards the second contract as a 'slight variant' of the first. I have called the type of argument of which Professor Popper's 'first contract' is a betting analogue 'unconditional simple induction', and that analogous to the second contract 'conditional simple induction'. Careful readers of my book will learn that the logical foundations of these two types of reasoning are substantially different.

I

By the 'first contract' two gamblers, Peter and Paul, who are to proceed across a desert of unknown dimensions, agree in advance that at every minute Peter incurs a debt of $x/(x+1)$ pennies to Paul if the end of the desert is reached within the next m/x minutes, while Paul incurs a debt of $1/(x+1)$ pennies, if the end is not reached within the next m/x minutes, where x is any number agreed on in advance and m is the number of minutes for which they have so far at the end of each minute been on the desert; thus m grows as they proceed. (A slight error is involved by taking a finite stretch of time, a minute, but this does not matter if a minute is small in relation to the length of the journey.) The probability p of not reaching the end of the desert within the m/x minutes, which grows as they proceed, is $x/(x+1)$.

Of the first contract Professor Popper remarks 'that p is nothing but the proportion of the number of games which Peter is bound to win to the total number of games to be played under the contract'.[1]

What, I submit, is remarkable about this is that, in the absence of all knowledge about the size of the desert, it is possible *in advance* to define the terms of a prediction (or betting contract), so that it is certain that the prediction will be true a specified proportion of the times that it is made; this means, as Professor Popper admits, that the betting contract is ' fair ', in the sense that Peter and Paul will necessarily be all square at the end of the journey. By making x large enough, we can make that specified proportion of the number of times that the prediction will be true to the total number of times that it is made as high as we like.

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[1] The betting analogue does not allow, as I do, for the desert being of infinite extent (or outlasting the lives of Peter and Paul). The probability of its not coming to an end in m/x minutes is stated by me as *at least* $x/(x+1)$, while I state nothing about the probability of its coming to an end. I have been most careful to point out that the inductive probabilities of 'X' and 'not X' do *not* normally add up to 1. Although a modification is in fact required in Professor Popper's 'first contract' to allow for the possibility of an extent being infinite (or outlasting the lives of Peter and Paul), I do not judge that there is any need to complicate the argument of the text by introducing it there. But in practice the modification is of the utmost importance, since by far the most important stretches, for whose continuance into the future we rely on simply induction (viz. continued operation of laws of nature, etc.), we do in fact expect to outlast our own life time.

My submission is that the fact of our being able to draw up in advance the terms for a prediction of fact by which it must be true a high proportion of the times that it is made *without any prior knowledge of any particular facts* is what validates induction. It substitutes reason for a mere hunch as the basis for that confidence that philosophers have had for many centuries that past experience is a good guide to what will happen in future.

I am unable to see the force of the only comment that Professor Popper makes on the ' first contract '. He says that if Peter and/or Paul knew in advance the extent of the desert, but agreed—rather fatuously—on this betting arrangement, they would be all square. He adds, ' it follows from this that the probability for which Mr Harrod *correctly* calculates *cannot be a probability that may be interpreted as the degree of Peter's imperfect knowledge* ' (italics Professor Popper's).

As I can see no force in this, I can only argue about it by an analogy. I warn the reader heavily that in the analogous case, which I take because it is familiar, prior knowledge is assumed, whereas there is no prior knowledge in the case of the desert; the difference does not affect the analogy. Let Peter and Paul have a pack of cards before them which—somehow or other—they know to be normal. Let them agree in advance to turn over all the cards and that, when a red card is turned, Peter gives Paul a penny, and, when a black card is turned, Paul gives Peter a penny. They will be all square at the end. We say that the probability of P winning at the turn of any particular card is $\frac{1}{2}$. p is nothing but the proportion of the number of turns at which Peter is bound to win to the total number of turns. Now let us suppose an additional piece of knowledge, namely, that Peter and/or Paul knows the order in which the cards have previously been arranged. If in these circumstances they proceed to bet as before, they will be all square at the end. How does this fact affect the validity of saying that, if neither knows the order, the probability of each winning at each turn is $\frac{1}{2}$?

[illegible]

I hold that all arguments in probability implicitly refer the case in question to a class of cases. In the illustration just given of simple induction (analogous to a ' first contract ' type of bet), the class is all the predictions (bets) made during the journey. The prediction is true $x/(1 + x)$ of the times that it is made.

In the ' second contract ' Peter and Paul agree to have one bet only, namely after the journey has proceeded for m minutes, the value of m to be determined by the drawing of a ball marked with a number from an urn, after the agreement is made, but before the journey has begun. In this case too, according to Professor Popper, the probability that the journey will last for another m/x minutes is still $x/(x + 1)$. (If m happens to be greater than the extent of the desert, the bet is called off.) Surely the sagacious reader will perceive that this is an altogether different kind of case.

It is analogous to my ' conditional simple induction ', except for the reference to the urn. Incidentally I object to urns because they are closely associated with traditional probability theory, which has done so much to bedevil inductive probability theory. In this case I doubt if the argument can be developed without the assumption that one is equally likely to draw any number from the urn, or, anyhow, that there is no correlation between the number on the ball drawn and the length of the journey. This is violently objectionable, because it contravenes the assumption of total nescience.

One can simplify Professor Popper's second contract by substituting for the drawing from an urn an agreement that the bet becomes operative at the minute that either of them first says BO. (If neither says BO before the end, the bet is called off.) We assume of course that neither acquires any clue as to the extent of the desert except that it is larger than the part already transversed.[1] By the same reasoning as that which Professor Popper applies to the case of the urn drawing, we can say that p at the moment BO is said is $x/(x+1)$.

Now what is the class of predictions (or bets) of which such a prediction is a member? It can no longer be the class of predictions made during the journey, because no other predictions are made besides this one. We have, rather, to assume that Peter and Paul do this kind of thing rather often and each time utter BO once. The probability class becomes the total number of times that they utter BO and this must be taken to be fairly large.

Conditional simple induction, in this unlike unconditional simple induction, requires what I have called a ' fair sampling postulate '. The need for this postulate and the justification for it in this particular instance of its use are discussed on pages 65-66 and 73-78 of my book. The fact that Professor Popper's second contract requires this postulate, while his first does not, means that they are logically rather considerably different from one another. It is no doubt my fault that Professor Popper can have read my book and still thought that the one is but a ' slight variant ' of the other.

3

A man from Mars, who knows nothing of the nature of telegraph poles or roads, is parachuted down on to a British road, and, having proceeded for a yard, comes to a telegraph pole and again once every ten yards. At the end of 99 yards, having transversed a poleless track of 8 yards, he may be disposed to use my formula to lay odds *against* a pole appearing in the next four. Or, thinking that for 91 yards the road has had the character of being poled once every ten yards, be may be disposed, again using my formula, to lay odds *in favour* of a pole appearing in the next four. ' Yet ', says Professor Popper, ' these two probabilities contradict each other '. That is not surprising, since the two dispositions, as noted, are based on different evidence, one on eight yards without a pole and the other on 91 yards with poles spaced at 10 yard intervals. Probability consists essentially in the relation of a proposed inference to the evidence on which it is based. Accordingly it is not surprising that probabilities based on different evidence contradict each other. What is surprising is that Professor Popper reaches the opposite conclusion to that which one could have expected. Instead of saying that one expects different degrees of probability for an event when different evidence is used, he says that ' the odds refer to entirely different gambling contracts (to different *relative* probabilities)'. [What does the bracketed phrase mean?] ' Yet the paradox establishes that Mr Harrod's always applicable method of calculation cannot possibly be interpreted as referring to inductive probabilities.' He also states that this 'simple paradox appears to be fatal for Mr Harrod's inductivist interpretations of probabilities'.

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[1] I am not dealing here with the significance of this accrual of knowledge, as it is dealt with extensively in my book and does not affect the present argument.

It is surely a general principle of inference that one is more likely to be right if one uses all the evidence in a particular case than if one uses part of it only. If the visitor from Mars has a short memory that does not extend beyond eight yards, he will none the less be on a good wicket if he uses his formula ('second contract' type of case) whenever he finds himself having travelled on a British road with no pole for eight yards, although he will be wrong on the first occasion.

Even if all British roads were poled at ten yard intervals, he would have a majority of successes in a wider class of cases, e.g. including journeys over open fields. Of course if nature was nothing but a wide open stretch with a network of poles of ten yard mesh, this unfortunate individual with his eight yard memory, whose mental equipment happened to be so much out of accord with the nature of his environment, would soon conclude—if only he could remember enough—that the fair sampling postulate did not apply to his experience. Even he, however, would have a majority of successes in the 'first contract' type of case (which does not require the fair sampling postulate), viz. if he continuously predicted 'no pole in future for at least half of the poleless tract that I have already traversed'.

In the case of this paradox, the larger evidence comprises the more restricted (assuming sufficient memory); the evidence of the ninety-nine yards journey contains the evidence of the last eight yards of it; accordingly the longer stretch should be used as evidence. The problem where there are *independent* items of evidence, both relevant, yielding different probabilities is a more difficult one; I do not believe that anyone has dealt with it satisfactorily; that is because it is difficult; it does not look at all like a problem in principle insoluble. I did not venture to treat of it in my book. But perhaps some day I may do so.

Roy Harrod

On So-called Degrees of Confirmation

A controversy flared up some years ago between Carnap, Popper, Bar-Hillel, and Kemeny, which I should like to re-open here. In his *Logical Foundations of Probability*, published, the reader may recall, in 1950, Carnap had interpreted $p(h, e)$, where p is a probability function, as the degree to which h is confirmed by e.[1] Four years later Popper questioned Carnap's interpretation of $p(h, e)$, which he dismissed as beyond repair.[2] Carnap, Bar-Hillel, and Kemeny subsequently rose to meet the challenge, without, however, satisfying Popper who, as of early 1959, still stood his ground.[1] Now in my opinion Popper was right in questioning Carnap's interpretation of $p(h, e)$, a point which his opponents have failed to grant him; he was wrong, however, in dismissing it as beyond repair. The whole matter may therefore be worth reviewing.

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[1] See *Logical Foundations of Probability*, Chicago, 1950, sections 8-10 and 41A. It should be pointed out that Carnap offers in section 41 of his book two other interpretations of $p(h, e)$ which I understand him to prefer now to the one under discussion. Carnap of course writes '$c(h, e)$' where I have '$p(h, e)$'. At the editor's suggestion, however, I substitute the neutral letter 'p' for the letter 'c', use of which might seem question-begging here.

[2] See 'Degree of Confirmation', this *Journal*, 1954, 5, 143 ff. See also Popper's *The Logic of Scientific Discovery*, New York, 1959, Appendix *ix, where the above paper and two additional ones also published in this *Journal*, 1957, 7, 350 ff., and 1958, **8**, 294 ff., are reprinted and commented upon.

Let a function p defined for pairs of sentences from a formalised language L be said *to be a quasi-regular probability function* if it satisfies the following requirements:

R1: $p(h, e) \leq 1$;

R2: If e logically implies h, then $p(h, e) = 1$;

R3: If e and i are logically equivalent, then $p(h, e) = p(h, i)$;

R4: If e is not logically false and e logically implies $\sim (h \;\&\; i)$, then $p(h \vee i, e) = p(h, e) + p(i, e)$;

R5: $p(h \;\&\; i, e) = p(h, e) \times p(i, e \;\&\; h)$;

and let a like function p be said *to be a regular probability function* if it satisfies R1-R5 and the following requirement:

R6: If $p(h, e) = 1$, then e logically implies h.[2]

There are two objections to interpreting $p(h, e)$, where p is either a quasi-regular or a regular probability function, as the degree to which h is confirmed by e. One has been made by several writers, myself included.[3] The other, which sparked the controversy I just described, is due to Popper.

(1) It makes little sense—on the face of it, at least—to talk of the degree to which a factual hypothesis h is confirmed by a logical truth, say lt, and, were p allowed to have a value for such a pair of sentences, one might well wish p to have the same value for any two such pairs. How indeed could the degree to which one factual hypothesis h_1 is confirmed by lt, granting that there is such a thing, be higher or lower than the degree to which another factual hypothesis h_2 is confirmed by lt? Yet not only do R1-R5, on one hand, R1-R6, on the other, allow p to have a value for a factual hypothesis h and lt, but they allow that value to vary from one factual hypothesis h to another.

(2) A probability function p (which may be indifferently assumed to satisfy either R1-R5 or R1-R6) and a pair of sentences h and e may be constructed such that $p(h, e) > p(h, lt)$, $p(\sim h, e) \leq p(\sim h, lt)$, and $p(h, e) < p(\sim h, e)$. If one understands by '$p(h, e) > p(h, lt)$' that e is positively relevant to h, then one is left with a pair of sentences h and e such that e is positively relevant to h, is not positively relevant to $\sim h$, and yet confirms h to a lower degree than it confirms $\sim h$, a disturbing enough result. If, on the other hand, one understands by '$p(h, e) > p(h, lt)$' that

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[1] Carnap's and Bar-Hillel's answers to Popper will be found in this *Journal*, 1955, **6**, 155-157, 1956, **7**, 243-244, and 245-248. Kemeny's answer will be found in *The Journal of Symbolic Logic*, 1955, **20**, 304. For Popper's present view of the matter, see *The Logic of Scientific Discovery*, pp. 387-395.

[2] The present definitions of a quasi-regular and of a regular probability function are borrowed from Carnap's *Notes on Probability and Induction*, U. C. L. A., 1955, pp. (3)-(4), where Carnap of course has 'confirmation' where I have 'probability'. The right-hand argument of p is allowed to be logically false in accordance with my paper 'On Logically False Evidence Statements', *The Journal of Symbolic Logic*, 1957, **22**, 345-349.

[3] See, for instance, my paper 'Evidence logique et degré de confirmation.' *Revue Philosophique de Louvain*, 1954, **52**, 619-625.

e confirms h, then one is left with a pair of sentences h and e such that e confirms h, does not confirm $\sim h$, and yet confirms h to a lower degree than it confirms $\sim h$, an even more disturbing result.[1]

Both difficulties would vanish, it seems, if, $p(h, e)$ being still interpreted as the degree to which h is confirmed by e, logical truths were kept from serving as right-hand arguments to the function p, a matter which could be arranged through the insertion of appropriate restrictions in R1–R5 or R1–R6. True, a probability function p (which may be indifferently assumed to satisfy either R1–R5 or R1–R6) and a triple of sentences h, i, and e (reminiscent of Popper's pair h and e) may also be constructed such that $p(h, e \,\&\, i) > p(h, e)$, $p(\sim h, e \,\&\, i) \leq p(\sim h, e)$, and $p(h, e \,\&\, i) < p(\sim h, e \,\&\, i)$. It is easily shown, however, that whenever a sentence i is positively relevant to another sentence h given a third sentence e, i is not positively relevant to $\sim h$ given e, and yet the degree to which h is confirmed by $e \,\&\, i$ is lower than that to which $\sim h$ is confirmed by $e \,\&\, i$, then h must be confirmed by e alone to a lower degree than $\sim h$ is, which of course clears Carnap's interpretation of $p(h, e)$ (e being assumed not to be logically true) of any suspicion.[2]

Both difficulties would also vanish if, logical truths being still allowed to serve as right-hand arguments to p, $p(h, e)$, where p is a quasi-regular probability function, were re-interpreted as the degree to which h is confirmed by some unspecified evidence, call it UE, plus the evidence (if any) conveyed by e. $p(h, lt)$ would simply become the degree to which h is confirmed by the evidence UE in question and none other. As for Popper's counter-example, e, though adding to the degree to which h is otherwise confirmed by UE alone and not adding to the degree to which $\sim h$ is otherwise confirmed by UE alone, would in conjunction with UE confirm h to a lesser degree than it does $\sim h$ simply because UE alone confirms h to a lesser degree than it does $\sim h$.

I was careful in the last paragraph to insist on p being a quasi-regular rather than a regular probability function. If indeed $p(h, e)$ is re-interpreted as the degree to which h is confirmed by UE and the evidence (if any) conveyed by e, then $p(h, e)$ may equal 1 without e alone having to imply h and hence R6 may fail.[3] This contretemps need not, however, be so fatal.

Regular probability functions have met with favour because they can be defined in terms of null probability functions and *vice-versa*. Let indeed a function p_0 defined for individual sentences from a formalised language L be said *to be a null probability function* if it satisfies the following requirements:

R_01: $0 \leq p_0(h)$;
R_02: (*a*) If h is logically true, then $p_0(h) = 1$; (*b*) If $p_0(h) = 1$, then h is logically true;
R_03: If h and i are logically equivalent, then $p_0(h) = p_0(i)$;
R_04: If $\sim(h \,\&\, i)$ is logically true, then $p_0(h \vee i) = p_0(h) + p_0(i)$.

It can be shown that (1) $p(h, lt)$, where p is a regular probability function, satisfies each one of R_01–R_04, that (2) $p_0(h \,\&\, e)/p_0(e)$ in case $p_0(e) \neq 0$, 1 in case $p_0(e) = 0$, where in either case p_0 is a null probability function, satisfies each one of R1-R6, and hence that (3) the two families of functions p and p_0 are interdefinable.

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[1] In Popper h is taken to read 'Six will turn up', e to read 'An even number will turn up', $p(h, lt)$ is made to equal 1/6, $p(e, lt)$ to equal 1/2, and hence $p(h, e)$ to equal 1/3; see *The Logic of Scientific Discovery*, p. 390.

[2] The three sentences in question will be found in Carnap's *Logical Foundations of Probability*, sections 69 and 71.

[3] This point was brought to my attention by Professor Carnap.

When $p(h, e)$ is interpreted as the degree to which h is confirmed by UE and the evidence (if any) conveyed by e, $p_0(h)$ must be interpreted as the degree to which h is confirmed by UE alone and, hence, both R6 and R_02(b) fail, which blocks the above results (1)-(3). Imagine, however, that instead of satisfying R1-R6, a regular probability function p is merely expected to satisfy R1, R2, R3, R5, and the following consequences of R4 and R6, both of which hold under our re-interpretation of $p(h, e)$:

R4′: If $p(e, lt) \neq 0$ and e logically implies $\sim(h \ \& \ i)$, then $p(h \vee i, e) = p(h, e) + p(i, e)$;

R6′: If $p(e, lt) = 0$, then $p(h, e) = 1$.

Imagine also that, instead of satisfying R_01-R_04, a null probability function p_0 is merely expected to satisfy R_01, $R_02(a)$, R_03, and R_04. Then the two families of functions p_0 and p will again be interdefinable, as the reader may check.

The twin notions of a regular probability function and a null probability function can thus be so modified as to suit my re-interpretation of $p(h, e)$ and permit regular probability functions to be defined in terms of null ones and *vice-versa*.

Because of the above I conclude that Carnap's interpretation of $p(h, e)$, though liable in the original to Popper's criticism as well as others, is not beyond repair.[1]

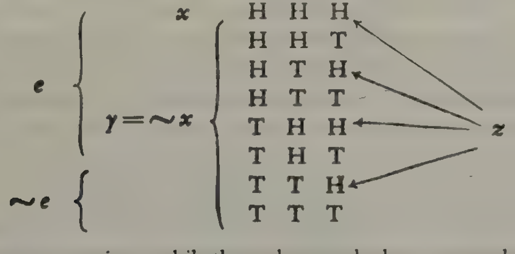
Bryn Mawr College HUGUES LEBLANC

PROBABILISTIC INDEPENDENCE AND CORROBORATION BY EMPIRICAL TESTS

I AM glad that Professor Leblanc has reopened the discussion of my criticism of the thesis (developed in much detail by Jeffreys, Keynes, Carnap, and others) that the degree of empirical corroboration or confirmation of a theory can be adequately represented by degrees of probability, in the sense of the probability calculus.

In his note Professor Leblanc does not attempt to challenge my arguments (given in the papers to which he refers and in my *Logic of Scientific Discovery*). He hopes, however, that by a modification of the calculus of probability, the absurdities which I pointed out can be avoided. I am not averse to the modification he proposes to adopt (it has also been suggested by Jeffreys and Keynes and, from a very different point of view, by A. Rényi). Yet his means cannot achieve his ends. The modification fails to save the position which he is so anxious to defend. It is, indeed, an attempt 'to stop the ocean with a bulrush', to quote Hume (*Natural History of Religion*, end of section ix).

1 Let us consider the three next tosses with a certain penny. There are eight possibilities of 'heads turning up' (H) and 'tails turning up' (T)



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[1] This paper was written while the author worked on a research project sponsored by the American Philosophical Society.

Now let x be the statement HHH, that is, ' heads will turn up thrice '; let y be the statement $\sim x$ (non-x); let z be the statement ' the last toss turns up heads '; and let $\sim e$ be the statement ' the first two tosses turn up tails '. We then have the probabilities among others:

$$p(x) = 1/8 \qquad p(y) = 7/8 \tag{1}$$

$$p(x,e) = 1/6 \qquad p(y,e) = 5/6 \tag{2}$$

$$p(x,z) = 1/4 \qquad p(y,z) = 3/4 \tag{3}$$

$$p(x,z.e) = 1/3 \qquad p(y,z.e) = 2/3 \tag{4}$$

We see that each of the ' evidence statements ' z and e, *supports* x, in the sense of raising its probability, while *undermining* y. And we see, further, that the evidence statements z and e, singly or in combination, support x and undermine y.

In some of my earlier publications[1] I argued as follows: the evidence statements support x and undermine y. It would therefore be absurd to say that y is better, or to a higher degree, confirmed by them than is x. Or if we use ' $C(\ldots, \ldots)$ ' for ' degree of confirmation ', I asserted that

$$C(x,z) < C(y,z) \tag{5}$$

$$C(x,e) < C(y,e) \tag{6}$$

$$C(x,z.e) < C(y,z.e) \tag{7}$$

would all be absurd, in view of the fact that the evidence statements z and e *support* x and *undermine* y. On the other hand, we do have

$$p(x,z) < p(y,z) \tag{5'}$$

$$p(x,e) < p(y,e) \tag{6'}$$

$$p(x,z.e) < p(y,z.e) \tag{7'}$$

in view of (2), (3), (4); which shows that it is absurd to identify C with p, or degree of confirmation with probability.

2 Professor Leblanc admits the cogency of my argument, but he hopes to avoid it by forbidding, as meaningless, what I call ' absolute probabilities ', such as (1), and permitting only what I call ' relative probabilities ', such as (2) to (4). Let us therefore forget about (1), and concentrate on (2) and (4); in other words, let us consider only the probabilities ' given the evidence e ' and ' given the evidence z and e'.

We then see that, in the presence of e, z supports x and undermines y, and that it would be therefore absurd to say that, in the presence of e, y is better confirmed than is x.

But according to Professor Leblanc, there should be no difficulty here: all we have to assume is that e supports y and undermines x, and that it does this so powerfully that y remains better confirmed even after the undermining evidence z has been added. The intuitive impression that e supports x rather than y, and undermines y rather than z, must be disregarded; and it can safely be disregarded since we have forbidden (1) as meaningless; and it is only by comparing (2) with (1) that we can say that e supports x rather than y.

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[1] This *Journal*, 1954, 5, 143 ff., 334, 359; and *The Logic of Scientific Discovery*, London, 1959, pp. 387-402. I have chosen my present example so that the probabilities in the presence of e coincide numerically with the probabilities without e of the example on p. 390 of my book.

But ostrich policy never pays. It is not only by comparing (2) with (1) that we can discover that e supports x and not y. We can find this out by comparing the effect of the information e with the effect of the information $- e = w$. For we find

(8) $p(x, \sim e) = 0$ $p(y, \sim e) = 1$

while we had

(2) $p(x,e) = 1/6$ $p(y,e) = 5/6$

Comparing these two lines, we see that e supports x and undermines y, and we see this without reference to the absolute probabilities (1).

3 Professor Leblanc has, in fact, deduced a new absurdity, in addition to those I have deduced before, from the identification of C with p, or of confirmation or corroboration with probability. For he shows that he is *a priori* committed to the assertion that e confirms y better than x, however we may choose e. For he derives from the assumption that the equations (2) and (4) hold the following conclusion: 'x must be confirmed by e alone to a lower degree than y is, which of course clears Carnap's interpretation of $p(x,e)$ (e being assumed not to be logically true) of every suspicion.'[1]

But it is more than suspect to assert, *a priori*, of every e which we may choose that it supports y rather than x. For in fact, we have great latitude in our choice of e. We may choose e so that $p(x,e) = p(x, \sim e)$ and $p(y,e) = p(y, \sim e)$; that is to say, we may choose e so as to be *independent* of x and y. (In this case we can define the absolute probabilities of x and y by $p(x) = p(x,e)$ and $p(y) = p(y,e)$.) We can achieve this, say, by putting $e =$ 'at the last toss yesterday with this coin heads turned up'. Or we may do better still, as I have shown by my example, and choose e so as to support x and undermine y. Professor Leblanc's result—that, if (2) and (4) hold, e must always undermine x and support y—exhibits therefore a new absurdity as a consequence of the identification of C and p.

4 Let us not quarrel about words. There is nothing magical in the word 'probability' but it is conducive to clarity if we agree that we call 'probability' all those things, and only those things, which satisfy the well-known 'calculus of probability'. (I have given axiom systems for this calculus in this *Journal*, in my *Logic of Scientific Discovery*, and in other places.) There is nothing magical about the words 'degree of confirmation'; and there would be no objection to calling the logical probability of x given y 'degree of confirmation of x given y'—were it not for the fact that the same people who propose to call logical probability 'degree of confirmation' mistakenly believed that it could measure the degree to which y confirms or corroborates x. Keynes, Jeffreys, Hosiasson, and Carnap made it all clear that they thought that a probability function $p(x,y)$ could measure the degree to which x is *empirically confirmed or corroborated or supported or tested* by the empirical evidence y.

But this has been shown by me to be an impossible interpretation of $p(x,y)$ in the sense of the calculus of probability.

Of course, one can now back out of this by a small change of terminology. One can drop the talk of x being confirmed '*by*' y and speak of 'x being confirmed, "*given*"

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[1] See text to Professor Leblanc's second footnote. I have changed back his notation to mine.

y' or being confirmed '*in the presence* of' y. The change is so small that few people will notice it. But it amounts to saying that we do no longer speak about the degree to which x is confirmed *by* the empirical evidence y. And if this change of meaning is to be carried out, it will be necessary to abandon Carnap's fundamental suggestion for the 'classificatory' concept of confirmation; I mean the suggestion that y confirms x if, and only if, y raises the probability of x, so that

$$p(x) < p(x,y)$$

By abandoning this suggestion, one would have to abandon any connection of 'confirmation' with empirical support, or empirical corroboration.

Professor Leblanc is mistaken in attributing to me the view that Carnap's interpretation of probability by confirmation is irreparable. It clearly cannot be repaired by Professor Leblanc's method. But it can be 'repaired' by giving up the original plan of measuring empirical corroboration.

K. R. Popper

Confirmation Without Background Knowledge

Although it is against Dr J. Agassi that Mr H. G. Alexander's latest remarks (this *Journal*, 1959, 39) are directed, the fact that I was his original culprit and that he now devotes Section 1 to a discussion—'tedious' but necessary—of what I really had in mind in the articles he criticised on a previous occasion (this *Journal*, 1958, 35) give me an excuse for butting in. Moreover, it so happens that a statement of what I had in mind will, I believe, lead to a refutation of the claim he makes in Section 2, namely that the so-called 'paradoxes of confirmation' are generated not only by an instantiation-theory of confirmation (which I attacked) but also by a testability-theory (which I espoused) if background knowledge is excluded.

Did I, when I wrote those articles, realise that non-ravens may provide supporting evidence for 'All ravens are black'? I did. I realised that some objects, such as photographs, known to be non-raven, might nevertheless provide evidence about the colouring of ravens (hence the intentional absence of commas in the last sentence in the passage Alexander re-quotes in his reply). But the more interesting case, which I did not have in mind, is the rare one of non-ravens suspected of being ravens. If I *suspect* that a greyish-looking object flapping in the twilight is a raven, then I know that closer investigation of it *may* lead to the falsification of 'All ravens are black': our hypothesis will have survived a test if this object turns out to be black, or no raven, after all. Thus the object might provide supporting evidence even if it was in fact no raven at all.

Agassi's Note (this *Journal*, 1959, 36) brought out the importance of this sort of experimental confirmation. Disdaining the philosophical aviary of ravens and swans, he drew from the history of science examples of a hypothesis being confirmed when an experimental situation which appeared to satisfy its antecedent clause, and which clearly failed to satisfy its consequent clause, turned out on closer investigation not to satisfy its antecedent clause after all. But there is all the difference between this sort of situation and one which was *already known* not to satisfy its antecedent clause. No amount of further investigation of this latter sort of situation could lead to the falsification of our hypothesis, or provide a test of it, or (on a testability-theory of confirmation) confirm it.

Alexander argued that if, on a testability theory, a raven unsuccessfully examined for non-blackness confirms ' All ravens are black ', then any non-black object (a grey elephant, say) unsuccessfully examined for ravenhood must also confirm the equivalent hypothesis ' All non-black objects are non-ravens '—and (we may add), any object whatever (a dinner-jacket, say) unsuccessfully examined for being not *black-or-no-raven* must confirm the equivalent hypothesis, ' Everything is black or no raven '. Thus a testability theory, just as much as an instantiation-theory, makes *every* object relevant to the confirmation of our hypothesis *if prior background knowledge is excluded*, since in that case the notion of testing becomes so vague that *any* observation may count as a test; thus I cheated when I claimed that one way in which Popper's testability-theory is superior to Hempel's instantiation-theory is that it avoids the counter-intuitive consequence that elephants and dinner-jackets *must* constitute confirming evidence for ' All ravens are black '; for it avoids it only if background knowledge (which, Alexander maintains, Hempel explicitly excluded) is covertly re-introduced.

This whole claim of Alexander's is mistaken. Suppose I am entertaining the hypothesis ' All ravens are black ', and have *only* the following factual information: object *a* is known to be grey and not known to be no raven; *b* is known to be a raven; *c* is known to be no raven; *d* is known to be black. (To forestall an objection: ' *a* is known to be grey ' is not a lapse into psychologism, though ' I feel certain that *a* is grey ' might be.) Then, without any background beliefs about the relative probabilities of an object being a raven and being non-black, etc. (see section 3 of Alexander's *Reply*), I know that further investigation of *a* and *b* might lead to the falsification of our hypothesis and would provide a test of it. I also know that further investigation of *c* and *d could not possibly* lead to its falsification and hence that the results of such an investigation, whatever they might be, could not confirm it according to a testability-theory of confirmation. But on an instantiation-theory, both *c* and *d* (which might be an elephant and a dinner-jacket respectively) instantiate, and therefore ' confirm ', our hypothesis.

This example proves that on a testability-theory it *may* be that a grey handkerchief does, while a black raven does not, provide confirmation for ' All ravens are black.' For *a* might turn out to be a grey handkerchief while *d* might have been a black raven.

Thus Alexander is wrong when he says, ' So long as one accepts this proviso [viz. that background knowledge be excluded] one will be unable to avoid the counter-intuitive consequences ' (p. 232). A testability-theory of confirmation would still avoid them even if this proviso were accepted. (Incidentally, Alexander must be wrong when he suggests that Hempel introduced this proviso because he wanted to offer a rational reconstruction of the way science originally develops from nothing but rock-bottom singular observation-statements, for Hempel explicitly rejects this naive view of the growth of knowledge, agreeing with Popper (*Mind*, 1945, pp. 4-5) that scientific hypotheses may be prior conjectures. Hempel's problem was: *given* a hypothesis, when do observation statements confirm or disconfirm it. I am sorry that Alexander is ' very tempted to follow Hempel ' in a direction which Hempel actually, and quite rightly, refused to take.)

As to Alexander's Section 4, the aim of reconciling opposing views is often praiseworthy in politics, but it will only obscure the issues here. The differences between Popper's and inductivists' views are large, and, I should have thought, easily visible.

Perhaps one reason why Alexander does not see them is that he confuses Popper's solution of the problem of corroboration (When has a hypothesis proved its mettle?) with that of the problem of induction (How do we learn from experience?) Popper's solution of this second problem is: By *refuting* conjectures, inventing better ones, and *refuting* them. Has any inductivist offered a solution remotely resembling this? As for Popper's solution of the first problem, I fear that Alexander has not digested Agassi's remarks about the anti-authoritarian character of Popper's theory of corroboration. On this theory no scientific hypothesis, not even a low level experimental ' law ', is ever ' established ' by experiments which render further tests ' pointless ', to borrow two of Alexander's terms. On this theory, the experimental ' laws ' proposed by Kepler and Galileo, for instance, were indeed very well corroborated *but still entirely open to challenge*. As everyone knows, they were subsequently challenged and overthrown.

Alexander's inductivist spectacles have caused him to mis-see a good deal. I am inclined to attribute the fog he sees enveloping the notion of a test to the same cause.

I conclude that Alexander's attempt to make a testability-theory of corroboration generate counter-intuitive consequences fails. But I personally have benefited from examining his ingenious attempt. For me, the position is analogous to that sort of experimental corroboration mentioned earlier where what looked like counter-evidence turns out on further investigation not to be so after all. In such cases, a defender of the theory under attack should be grateful, not so much because instead of being refuted it is now better confirmed, but because he now has a better appreciation of its capacities.

J. W. N. WATKINS

REVIEWS

PROFESSOR BOHM'S PHILOSOPHY OF NATURE

I

THIS is a belated review of a highly interesting and thought provoking book.[1] Although dealing with some difficulties of a very specialised theory of today, viz. of the quantum theory, it should yet be of interest to the many non-physicists who want to know about the world we live in as well as about the ideas which are at present being developed for understanding this world. It is often assumed—and the basic philosophy of many contemporary physicists supports this assumption—that within the sciences speculation and ingenuity cannot play a very great rôle as physical theories are more or less uniquely determined by the facts. It is of course also assumed that our present knowledge about the microcosm is determined in exactly this way and therefore irrevocable, at least in its main features. The book shows that this is not correct, it shows that today there exists a clash of ideas about some very fundamental things, that the imposing and perhaps a little terrifying picture of science of an unalterable and steadily increasing collection of facts is nothing but a myth, and that ingenuity and speculation play in physics as great a rôle as anywhere else. It also shows that even now it is possible to present difficult matters in an interesting and understandable way. It shows thereby that the separation, so often deplored, between the sciences and the humanities is due to a false picture, if not a caricature of science. It is this false picture which is attacked throughout the book. More especially, the book contains an explicit refutation of the idea that complementarity, and complementarity alone solves all the ontological and conceptual problems of microphysics; that this solution possesses absolute validity; that the only thing left to the physicist of the future is to find, and to solve equations for the prediction of events which are otherwise well understood. In short, it contains a refutation of the idea that the physicist of the future is bound to be very similar to the more dogmatic of the medieval scholars with the sole exception that Bohr, and not Aristotle, will be his authority in matters metaphysical.

Secondly, the book presents, in qualitative terms, a new interpretation of some microphysical theories, and especially of the elementary quantum-theory of Schrödinger and Heisenberg. It attempts to develop, again in qualitative terms, a general picture of the universe which can give an account of statistical phenomena without assuming that they are irreducible. It discusses, on the basis of the picture presented, such fundamental problems of scientific method as the problem of induction, and the problem of the validity of empirical generalisations and of universal theories. Doing this without any discussion of ' ordinary language ' or of language systems it (implicitly)[1] refutes another idea that is very fashionable today, viz. the idea that the only fruitful way of discussing more general problems of knowledge is either to analyse ' ordinary ' language (whatever that may mean), or to construct formal systems and to investigate their properties.

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[1] *Causality and Chance in Modern Physics*, by David Bohm. Van Nostrand, New York; Routledge, London; 1957, pp. 170, $5 or 21s.

Having expressed in the above two paragraphs, as I hope I have done, that I consider Bohm's book a major contribution to the contemporary philosophy of nature I must at once add that there are many things in it which I cannot accept and that more especially his discussion of the problem of induction seems to me to be highly unsatisfactory. Bohm's physical ideas are original, refreshing, and sorely needed in a time of complacency with respect to fundamentals. But the philosophical standpoint taken up with respect to both physics and cosmology is traditional, and perhaps even reactionary: it is a curious mixture of the methodological doctrine of inductivism and of ideas which may be found in various dialectical philosophies. This will become evident from a more detailed investigation of the book.

2

In order to enter into Bohm's theory, I will first discuss the Copenhagen point of view. When it was first conceived this point of view constituted an interpretational feat of great importance. One realises this when the historical situation is considered a little more closely. The early quantum theory of Bohr and Sommerfeld, although experimentally very successful, was yet regarded as unsatisfactory by many physicists. Its main fault was seen to lie in the fact that it combined classical and non-classical assumptions in a way that made a coherent interpretation impossible. For many physicists it was nothing more than a stepping stone on the way to a really satisfactory theory, i.e. to a theory which could give us not only correct predictions, but also some insight into the nature and the dynamics of microscopic entities. It is quite true that Bohr, Heisenberg, and others worked along very different lines. Their main objective was not the construction of a new physical theory about a world that existed independently of measurement and observation; their main objective was rather the construction of a logical machinery for the utilisation of those parts of classical physics which could still be said to lead to correct predictions. Quite obviously a theory of this latter type does not admit of a realistic interpretation: the classical signs it contains cannot be interpreted realistically as they are no longer universally applicable. And the non-classical signs it contains cannot be interpreted realistically as they are elements of the logical machinery used for the purpose of prediction, and possess no meaning apart from that usage. However that may be—the philosophical spirit behind the ' Korrespondenzdenken ' was by no means shared by everybody. Now the most important thing is that Schrödinger's wave mechanics, which was conceived in an entirely different spirit, and which seemed to present the long awaited new and coherent account of the microscopic entities, encountered peculiar difficulties when the attempt was made to connect it with a universal interpretation of the kind that was applicable to the earlier theories. Any attempt to interpret wave mechanics as descriptive of entities which, although possessing new and surprising features, were still elements of an objective physical universe, any such attempt was found to lead to paradoxical consequences. It was Bohr's great merit that in this situation he developed an intuitive idea, the *idea of complementarity*, which, although incompatible with a straightforward realism, nevertheless gave the physicists a much needed intuitive aid for the handling of concrete problems.

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[1] Cf. also the explicit discussion of the merits of conceptual analysis in [156]. (*Note*: numbers in square brackets refer to pages of the book under review.)

According to this idea properties can be ascribed to a microscopic system only when it interacts with a suitable classical (i.e. macroscopic) piece of matter. Apart from the interaction the system possesses no properties at all. It is also asserted that the totality of classical measuring-instruments[1] divides into pairs of kinds which are mutually incompatible in the following sense: if the system under investigation interacts with a measuring instrument which belongs to one of two mutually incompatible kinds, then all the properties defined by interaction with the other kind will be wholly undetermined. And ' wholly undetermined ' means that it would be meaningless to ascribe such a property to the system just as it would be meaningless to ascribe to a fluid a certain value on the Mohs scale of scratchability. It is clear that the *uncertainty relations* now indicate the domain of permissible applicability of classical functors (such as the functor ' position ') rather than the mean deviations of their otherwise well defined values in large *ensembles*.

The idea of complementarity can be interpreted in two different ways. It can be interpreted as an attempt to provide an intuitive picture for an existing theory, viz. wave mechanics, and as a heuristic principle guiding future research. This interpretation is undogmatic as it admits the possibility of alternatives, and even of preferable alternatives. A physicist who looks at complementarity in this way will regard it as an interesting fact about quantum theory that it is *compatible* with a relational point of view where interaction is a necessary condition of the meaningful applicability of terms which within classical physics (relativity included) are definable without such reference. He will also point out that there exist no satisfactory alternatives. But he will never go as far as to assert that such alternatives will never be found, or that they would be logically inconsistent, or that they would contradict the facts. But Bohr's idea of complementarity can also be interpreted in a different way. It can be interpreted as a basic philosophical principle which is incapable of refutation and to which any future theory *must* conform. Bohr himself most certainly took this stronger point of view. ' Thus rather than consider the indeterminacy relationships primarily as a deduction from quantum mechanics in its current form he postulates these relationships directly as a basic law of nature and assumes . . . that all other laws will have to be consistent with these relationships ' [83, referring to Heisenberg]. His assumption was ' that the basic properties of matter can *never* be understood rationally in terms of unique and unambiguous models ' which implies that ' the use of complementary pairs of imprecisely defined concepts will be necessary for the detailed treatment of every domain that will ever be investigated ' [94]. It is true that some followers of the Copenhagen school have denied that this absolutism is part of complementarity. Thus in a discussion Rosenfeld has asserted that ' nobody thinks of attributing an absolute validity to the principles of quantum theory '.[1] But quite apart from the fact that he himself said in the lecture preceding this discussion that ' every feature ' of the theory ' is forced upon us ',[2] there is Bohr's explicit statement that ' it would be a misconception to believe that the difficulties of the atomic theory may be evaded by eventually replacing the concepts of classical physics by new conceptual forms '.[3]

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[1] This totality comprises pieces of matter which have not been prepared by a physicist for the purpose of measurement, but which, by accident, as it were, satisfy some very general conditions not to be discussed here.

3

This dogmatism with respect to fundamental principles is attacked and refuted in Chapter III of Bohm's book. The chapter contains an extremely lucid description of the development of the quantum theory and the various interpretations which have been suggested for it. It explains the resonable elements of the point of view of Bohr and Heisenberg. This point of view is presented with a clarity that is sadly missing in many writers who support Bohr, and with an understanding, and authority that reveals the former follower and expositor[4] of Bohr's ideas. The idea of its final and absolute validity is refuted by showing that all attempts to prove it (as indeed all attempts of a ' transcendental deduction ' of physical principles) are circular.

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[1] Rosenfeld, *Observation and Interpretation*, ed. Körner, London, 1957, p. 52

[2] Ibid., p. 41

[3] N. Bohr, *Atomic Theory and the Description of Nature*, Cambridge, 1932, p. 16

[4] Cf. Bohm, *Quantum Theory*, Princeton, 1951

Thus, in Heisenberg's 'proof' of the uncertainty principle (which is often used as an argument for its absolute validity)

> it was essential to use three properties; namely the quantization of energy and momentum in all interactions; the existence of these quanta; and the unpredictable and uncontrollable character of certain features of the individual quantum process. It is certainly true that these properties follow from the quantum theory [94].

However in order to show the basic and irrefutable character of the uncertainty principle these features themselves would have to be demonstrated as basic and irrefutable. Quite obviously such a demonstration cannot be achieved by pointing to some theorems of *wave mechanics* (such as von Neumann's theorems) as this would only lead to the further question whether wave mechanics is valid in all domains of experimentation [95]. Nor can it be achieved, as has been attempted by many inductivists, by utilising the fact (if it is a fact) that either wave mechanics, or some part of it, is highly confirmed. In order to see this most most clearly we need only realise that the assertion of the absolute validity of a physical principle implies the denial of any theory that contains its negation. More especially, the assertion of the absolute validity of the uncertainty principle implies the denial of any theory that ascribes to it only a limited validity in a restricted domain. But how could such a denial be justified by *experience* if the denied theory is so constructed that it gives the same predictions as the defended principle wherever the latter has been found to be confirmed by experience?[1]

It follows that neither experience nor mathematics can help if a decision is to be made between wave mechanics and an alternative theory which agrees with it in all those points where the latter has been found to be empirically successful. Now the idea of complementarity is well fitted to the structure of wave mechanics. As we cannot make any restrictions upon the structure of the empirically satisfactory alternatives of wave mechanics it also follows that its interpretation as a basic and irrefutable principle must be given up. Neither mathematics nor experience can be used to support such an interpretation. All this means, of course, that the position of complementarity is a metaphysical position[1] which can be defended by arguments of plausibility only.

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[1] Quantum mechanics is not the first theory that has been utilised for the purpose of excluding alternatives. Using the fact that certain theorems of Newtonian mechanics contradicted the second law of thermodynamics, Ostwald and Mach argued that a mechanical account of heat was impossible, and that Newton's laws could not be universally valid. It turned out, however, that it was the second law that was not universally valid (fluctuations). Quite clearly the Ostwald-Mach argument suffered from the same deficiency as the more recent arguments of Born, Rosenfeld, and others. They argued: the second law is highly confirmed; classical mechanics contradicts the second law; hence classical mechanics is not universally valid. They overlooked (*a*) that confirmation does not imply truth; (*b*) that the mechanical theory of heat contradicted the second law in a domain in which it had not yet been tested, and in which it was therefore neither confirmed nor disconfirmed.

4

So far only the (empirical and logical) *possibility* of alternative points of view has been shown. In Chapter IV of his book Bohm turns to the discussion of some alternatives that have actually been proposed in the literature and he also expounds some of his own ideas. I shall now give an outline of the epistemological background of all these alternatives.

One of the basic assumptions of the orthodox is that ' in our description of nature the purpose is . . . to trace down, as far as it is possible, relations between the manifold aspects of our experience '.[2] For them the facts of experience play the rôle of building stones out of which a theory may be constructed but which themselves neither can, nor should be modified. If we add to this the idea that ' only with the help of classical ideas is it possible to ascribe an unambiguous meaning to the results of observation '[3] (which means that the building stones referred to in the first quotation are classical states of affairs) we arrive at once at the result that a microscopic theory cannot be anything but a device for the prediction of a particular kind of fact, viz. of classical states of affairs. Now it is quite true that this point of view has led to some useful results (example: the dispersion formula of Ladenburg-Kramers; the first investigations of Heisenberg). It is also true that the quantum theory is the first theory of importance which to some extent satisfies the programme of Berkeley and Mach (classical states of affairs replacing the ' perceptions ' of the former and the ' elements ' of the latter). But it must not be forgotten that there is a whole tradition which is connected with the philosophical position of realism[1] and which went along completely different lines. In this tradition the facts of experience, whether or not they are now describable in terms of a universal theory (such as classical mechanics), are not regarded as unalterable building stones of knowledge; they are regarded as capable of analysis, of improvement, and it is even assumed that such an analysis and improvement is absolutely necessary. Indeed, the new theory of motion which was developed by Galileo and Newton could not possibly be understood as a device for establishing ' relations between the manifold aspects of our experience ', the simple reason being that, according to this very theory, observable motion would at best give us an approximation to its fundamental laws. Similarly the atomic theory of the late nineteenth century was not only not suggested, it was even contradicted by what was then regarded as an account of ' experience ', viz. classical thermodynamics. This tradition proceeds from the very reasonable assumption that our ideas *as well as* our experiences may be erroneous and that the latter give us at most an approximative account of what is going on in reality. Bohm's own point of view is closely connected with this tradition. Having shown that all the attempts to prove the uniqueness of the Copenhagen interpretation are invalid, he suggests ' to take the field and particle concepts of classical physics as starting points and to *modify* and enrich them in such a way that they are able to deal with the new combination of wave and particle properties that is implied in the quantum theory ' [98; my italics]. Such modified concepts, or even a completely new conceptual apparatus which does not any more make use of classical ideas, will of course at first be ' extraphysical ' [99] in the sense that it will not be accessible to test with the help of methods available *before* it was conceived. However ' the history of scientific research is full of examples in which it was very fruitful indeed to assume that certain objects and elements might be real, long before any procedures were known that could permit them to be observed directly ' [99]. Assumptions of this kind then

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[1] I use here the word ' metaphysical ' in the same sense in which it is used by the adherents of the orthodox point of view, viz. in the sense of ' neither mathematical, nor empirical '. That the Copenhagen interpretation is metaphysical in this sense has been asserted, in slightly different words, by Heisenberg who declared in 1930 (*Die physikalischen Grundlagen der Quantentheorie*, p. 15), that its adoption was a ' question of taste '. This he repeated in 1958 in the now more fashionable linguistic terminology (cf. *Physics and Philosophy*, New York, 1958, pp. 29 f). However at the very same place a highly objectionable criticism is found of Bohm's model of 1952. This model, it is asserted, ' cannot be refuted by experiment since [it] only repeat[s] the Copenhagen interpretation in a different language. From a strictly positivistic standpoint ' Heisenberg continues ' one may even say that we are here concerned not with counterproposals to the Copenhagen interpretation, but with its exact repetition in a different language '. Is it really the case that Bohm's counterexample against the assertion, made by von Neumann and others, that quantum theory does not allow for the addition of *untestable* hidden parameters (cf. von Neumann, *Mathematical Foundations of Quantum Mechanics*, Princeton, 1955, p. 326) is nothing but the ' exact repetition ' of this assertion ' in a different language '?

[2] Bohr, op. cit., p. 18

[3] Ibid., p. 17

> ultimately lead to new kinds of experiments and thus to the discovery of new facts. In the light of this historical experience [Bohm continues] positivism (i.e. the point of view expressed in the two above quotations) is seen to lead to a one sided point of view of the possible means of carrying out research. For while it recognizes the importance of the empirical data, positivism flies into the face of the historically demonstrable fact that the proposal of new concepts and theories having certain speculative aspects (e.g. the atomic theory) has quite frequently turned out to be as important in the long run as empirical discoveries have been [99].

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[1] For this connection cf. K. R. Popper's article ' The Aim of Science ', *Ratio*, 1958, 1

In this way positivism 'constitutes a dogmatic restriction of the possible forms of future experience' which in the case of quantum mechanics leads to the belief

> that the success of probabilistic theories of the type of the current quantum mechanics indicates that in the next domain it is very likely that we shall be led to theories that are . . . even more probabilistic than those of the current quantum domain [104].[1]

5

More concretely, Bohm's ideas as presented in the book under review may be regarded as an adaptation, to the case of the quantum theory, of the situation described by the classical kinetic theory of matter. The kinetic theory was an attempt to give an explanation, in terms of the motion of small, and as yet unobserved, particles, of the behaviour of thermodynamic systems. According to this theory continuous improvement of the precision of measurements will lead to the following phenomena (we assume that we move outside the domain where relativistic effects become noticeable): as long as we are dealing with large systems the classical laws of motion (and the second law of thermodynamics) will be found to hold with absolute precision. However when experimenting with fairly small systems such as dust-particles which are immersed into a surrounding medium, a completely new type of behaviour becomes apparent. These particles experience random displacements for which no explanation can be given in terms of the movements of bodies of a similar size. The laws describing this type of behaviour are not any longer the laws of classical mechanics. They are purely probabilistic and allow us to predict averages in large *ensembles* rather than individual processes. Within the framework of these laws no reason can be given for the occurrence of a particular movement of a particular particle. It can even be shown [107] that for particles under the conditions described above there exist laws which are formally identical with the uncertainty relationships, the diffusion constant of the embedding medium taking the place of Planck's constant h. But the situation changes again when we further improve the precision of our measurements or else use experiments of an altogether different type. We shall then find that the random behaviour of the dust particles is explainable in terms of a new set of causal laws referring to very small particles which are the ultimate constituents of the medium in which the dust-particles are immersed. (In the case of the kinetic theory these new laws happen to coincide with the laws of classical mechanics from which we started. However it is necessary to point out, in accordance with Bohm's more general ideas, that this need not always be the case.)

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[1] A terminological remark: quantum physicists have sometimes refused to be called 'positivists' on account of the fact that they accepted the Copenhagen point of view. Thus in *Niels Bohr and the Development of Physics* (London, 1955, p. 22) Heisenberg asserts that 'the Copenhagen interpretation . . . is in no way positivistic. For whereas positivism is based upon the sensual perceptions of the observer . . . the Copenhagen interpretation regards things and processes which are describable in terms of classical concepts . . . as the foundation of any physical interpretation.' This is quite true. However this 'foundation' is again assumed to be 'given' in the sense that it cannot be further analysed or explained, an attitude which to a certain extent still justifies the term 'positivism'.

Speaking more generally one may now say that according to the kinetic theory there exist three different levels of experimentation which are characterised by three different sets of laws. There is the macroscopic level where the laws of classical mechanics hold exactly. More precise experiments show then that these laws are not universally valid, and thereby delimit the domain of their applicability. At the same time they lead to a new set of laws governing phenomena which are *qualitatively* different from the phenomena we meet on the macrolevel, as they involve randomness. These new laws in their turn are not universally valid as they can be shown to be the result of the very complex, but again causal behaviour of entities on a still deeper level.

Now it is Bohm's contention that the situation in the domain of the quantum phenomena is similar to the one just described. As opposed to the opinion of the majority of physicists he assumes that the probability laws of the present quantum theory are the result of the very complex interplay of entities on a deeper level, and are therefore neither ultimate nor irreducible. Chapter IV contains a general discussion of various ways in which such a sub-quantum-mechanical level can be conceived. These considerations have been criticised by some members of the Copenhagen circle. One of the most frequent criticisms is that nobody has as yet succeeded in constructing a theory along these lines which can match the customary theory in predictive success. This criticism seems to proceed from the assumption that the existence of a certain theory and the absence of a theory, which is connected with a different 'ideology' as it were, may be regarded as an implicit criticism of the latter. However the fact that this pragmatic criticism can also be directed against the dynamical investigations of Galileo and Kepler (the successful theory being in this case Aristotle's theory of motion) should be sufficient to make its proponents a little more cautious about its force. A second criticism points out that the present theories, and the philosophical structure connected with them, are firmly based upon experience. This criticism has already been dismissed in an earlier part of the present review. Indeed, we have seen that the customary point of view about micro-physics cannot produce any empirical or logical argument against a procedure such as Bohm's. And assertions such as 'it is idle to "hope" that the cure of our troubles will come from underpinning quantum theory with some deterministic substratum' can at most be regarded as affirmations of faith.[1]

6

I leave now the physics of the book and turn to a discussion of the cosmology and methodology developed in it. Both these fields are dealt with on the basis of a generalisation of the situation described by the kinetic theory. The cosmological generalisation, as I understand it, is as follows: the world contains infinitely many levels. Each level is characterised by a set of laws which may be causal, or probabilistic, or both. The validity of these laws need not extend beyond the level to which they belong. When a certain level is left qualitatively new processes appear which have to be described by a new set of laws. Bohm recognises that sometimes these new laws may be general enough to allow for the derivation of the more specific laws of the preceding level (example: special relativity—general relativity; cf. [141]). However he points out—and this must be regarded as a highly important contribution to cosmology—that such a reduction need not always be possible. Let us assume, for example, that the level L_1 of causal laws possesses a substratum L_2 of probability laws which are the outcome of the causal interplay of entities of a level L_3 which in its turn possesses a probabilistic substratum L_4, and so on. Now the fact that the laws of L_2 can be explained by reference to complicated causal mechanisms on L_3 shows that they cannot be entirely random. On the other hand the laws of L_3 are not absolutely causal either as they are limited by the fluctuations which appear upon L_4. A complete explanation of the laws of L_1 (or of any set of causal laws or of probability laws) would therefore have to take into account an infinity of laws and levels. Clearly, then, an explanation of the laws of L_1 in terms of a finite sequence of substrata cannot be regarded as a *reduction* of L_1 to these substrata. Each level, and each set of laws possesses a surplus over and above any finite set of more general laws. It is only if we take all the mutually irreducible properties and laws together that we may hope to get a complete account of one particular level. This is the way in which Bohm makes physical sense of the idea of emergence and the irreducibility of qualities. At the same time it is suggested, at least by the cosmological model we are discussing at the present moment, that qualities may be reducible after all if only appropriate mathematical instruments are found for the handling of infinities of relatively self-contained experimental domains. The model also suggests a new interpretation of the difficult problems of probability, randomness, and statistical independence. In this interpretation neither the idea of a deterministic law, nor the idea of randomness is given absolute preference [20 f]. The laws of nature, whether they appear in the form of causal laws, or in the form of probability laws are regarded as a Hegelian synthesis, as it were, of the idea of absolute determination (the thesis), and of absolute randomness (the antithesis). This way of describing Bohm's procedure is by no means a mere verbal trick, for it is Bohm's conviction that in all fields the alternative use of opposite sets of concepts is to be preferred to the exclusive utilisation of only one of them.

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[1] Rosenfeld, in *Observation and Interpretation*, p. 44. In his review of the present book in the *Manchester Guardian*, L. Rosenfeld accused Bohm of contradicting the 'exigencies of sound scientific method' and he described the followers of Niels Bohr (and presumably also himself) as possessing the 'uncommitted, commonsense attitude of the true scientist'. Now first of all an attitude can hardly be called 'uncommitted' if it appeals to the principle that experience alone can be the judge of our theories, and at the same time is singled out neither by experience, nor by mathematics. Secondly the history of science has given ample evidence for the fact that it is 'sound scientific method' not to take experience at its face value, even if it should be expressed in very complicated (classical) terms, but to try to explain it as the result of processes which are not immediately accessible to observation. It is strange indeed to see that Rosenfeld describes as 'uncommitted' the attitude of those who because of their observationalistic bias distort both history and scientific method.

7

However, the model which we have just described and which plays an important rôle in Bohm's analysis of probability is not the one he uses in his discussion of scientific method. He is ' not even supposing that the general pattern of levels that has been so widely found in nature thus far must necessarily continue without limit'. He admits the possibility that ' even the pattern of levels itself will eventually fade out and be replaced by something quite different ' [139]. The structure of levels, he asserts, is only one way in which the *qualitative infinity of nature* may represent itself to the experimenter. This qualitative infinity of nature is one of the basic postulates of Bohm's cosmology. He incessantly insists upon the ' inexhaustible *depth* in the properties and qualities of matter ' [138] which is such that no finite system of laws and categories can ever express it adequately. Every thing and every process has infinitely many sides to it which are such that at any stage of scientific development they will only approximately be expressed by the laws and the concepts then in use. That such an approximate representation is at all possible is due to the further fact that there exists ' some degree of autonomy and stability ' in the mode of being of the things around us [139]. For example

> we may say that [a] real fluid is enormously richer in qualities and properties than is our macroscopic concept of it. It is richer, however, in just such a way that these additional characteristics may, in a wide variety of cases, be ignored in the macrodomain [155].

In spite of the fact that in every real fluid an infinite variety of processes is going on which are not covered by our macroscopic description of it, these processes just so counterbalance each other that relative stability is achieved upon the macrolevel, and the macroscopic description is in this way made applicable within its proper domain of validity. In short: the world is infinite as regards the properties and processes which are present in every part of it. But these properties are arranged in complexes of relative stability which may then be described with the help of scientific theories employing a finite number of concepts only. Every such description is true within a certain domain of validity. On the other hand the presence of further properties which are not covered by the description, and which slightly influence the elements of the complex implies that

> associated with any given law there must be errors that are essential and objective features of that law resulting from the multitude of diverse factors that the law in question must neglect. Thus each law inevitably has its errors, and these are just as necessary a part of its true significance as are those of its consequences that are correct [166].

It is important to repeat that for Bohm the errors referred to in the above quotation are not purely subjective phenomena; they possess an objective counterpart in the way in which the interplay between the elements of the relatively stable complexes as well as the qualities that have been left out delimits the validity of the laws describing the behaviour of the complexes. ' It is clear from the above discussion ' Bohm continues [166], ' that scientific research does not, and cannot lead to a knowledge of nature that is completely free from error. '

8

The application to *scientific method* is now quite straightforward. Nature is such that no law can ever be universally valid. Hence, it is sound scientific method to restrict the laws we find to a certain domain [135]. It is unsound method to apply them outside this domain. And never should we be so bold as to proclaim a certain law as universally valid, i.e. as valid in all domains of experimentation, and under all possible conditions. On the other hand, if we are careful enough in our pronouncements about the applicability of a scientific theory, and if we always restrict it to its proper domain, we do not run the risk of being refuted by new discoveries. For 'a new theory to which the discovery of . . . errors will eventually give rise, does not invalidate the old theories. Rather . . . it corrects the older theories in the domain in which they are inadequate and, in doing so, it helps to define the conditions under which they are valid ' [31]. Only a philosophical idea, and not sound scientific method can lead to the attempt to apply a theory to every possible domain. Thus the assumption ' that all the various levels, all qualitative changes, and all chance fluctuations will, eventually be reducible completely . . . to effects of some fixed . . . scheme of purely quantitative laws . . . is . . . essentially philosophical in character ' [62]. More especially the assumption that Newton's laws are universally valid

> has implications not necessarily following from the science of mechanics itself, but rather from the *unlimited* extrapolation of this science . . . Such an extrapolation is evidently . . . not founded . . . on what is known scientifically. Instead, it is in a large measure a consequence of a *philosophical* point of view . . . [37].

It is this methodological doctrine which I find highly questionable and which I shall attempt to criticise in the following last part of my review.

9

First of all, how does Bohm justify his two basic cosmological principles, viz. the principles of the infinity of nature and the second principle that there exist complexes which are relatively stable over a certain period of time and which therefore allow for the description, in terms of finite sets of laws and concepts, of parts of nature? The principle of the infinity of nature he tries to justify partly by reference to experience which shows us a great variety of qualities; partly by reference to the history of science which shows that every set of laws has at some time been found to be valid in a restricted domain only; and partly by reference to the ' basic spirit of scientific method itself, which requires that *every* feature be subjected to continuous probing and testing ' [132]. The principle of the existence of complexes of relative independence and stability is again justified by reference to experience; but it is also justified by some kind of ' transcendental ' reasoning according to which in a world of a different structure the concept of a thing would not be applicable and science would be impossible [139 f.]. Now if we look at these arguments we find that they are all unsatisfactory. To start with, Bohm's methodological rules which have been stated above would forbid us to draw consequences from experience and to apply them universally. Yet this is just what is done in the first argument. The appeal to the history of science cannot be accepted either. For it could also have been used by the Aristotelians *against* the assumption that human knowledge gave at most an approximate account of what went on in nature. Thirdly the transcendental argument is not of the slightest use as long as we do not know whether our theories express knowledge or whether they are not only well fabricated dreams. But knowing this would presuppose knowledge of exactly those states of affairs whose existence is to be proved with the help of the argument. And finally the methodological argument is of no help either as it might well be the case that all the tests we carry out with respect to a certain theory lead to its corroboration and thereby to the corroboration of the idea that the world possesses a finite number of basic properties after all. We see, then, that Bohm's two basic principles are not supported by the arguments he uses in their favour. They are not even empirical, or scientific in Bohm's own sense [cf. 166] as he is not prepared to admit that they may be valid in a certain domain only and give way to some kind of mechanicism in all the remaining domains of experimentation. They represent an *absolute truth* which is not capable of improvement by taking into account errors [169 f.]. Yet they are cosmological principles, i.e. principles describing the basic structure of our world. This, then, is my first criticism: that there is not the slightest reason for not treating the most general cosmological principles, such as the principle of the infinity of nature on a par with less general laws. There is not the slightest reason for denying them the status of all the other laws, viz. their provisional character.

10

However it seems to me that this criticism does not yet go to the heart of the matter. For it leaves out one of the most important arguments that Bohm could adduce in favour of the absolute character of his two principles. I did not find the argument in the book, but I trust that it may be constructed along the following lines. Consider a law that is valid in a certain domain only. When this law is properly stated we shall soon discover its limitations. We are able to do so because there exists another domain which is not covered by the law, and whose presence is responsible for the errors it possesses. The conditioned validity of the law and its approximative character are thus wholly dependent upon the objective existence of such other domains. It would then seem to follow that for lack of domains outside the domain of its applicability a statement about ' the infinite totality of matter in the process of becoming ' [170] must be unconditionally and absolutely valid. It is this argument which will be the starting point of my second criticism.

It is assumed in this argument that the provisional and approximative character of a scientific law is *wholly* due to the objective limitations of the stability of the entities, or of the domain it describes. We must correct the law not because we had a wrong idea about the properties of the things described. We must correct it because these properties themselves are the relatively stable result of a very complicated interplay of an infinity of processes, and because they are therefore subject to slight changes and transformations. *But if we keep well within the domain of application of the law, then we cannot possibly be mistaken.*

This last principle has the following very interesting corollary: every description of nature that has ever been uttered is true within its domain, and conversely, it exhibits the existence of a domain to which it properly applies. There does not exist any description that is wholly mistaken and without a corresponding reality. Or, to express it differently—when describing our surroundings *we always speak the truth* (relative truth, that is), *and we are also always in contact with some part of reality.* Now this corollary has so little *prima facie* plausibility that I must defend it before trying to show its shortcomings. ' Is it really the case ', one may easily feel oneself inclined to object 'that the savage who believes in, and claims to have observed, the actions of ghosts, tribal spirits, and the like is talking about entities which have some kind of existence in a restricted domain?' To this objection the retort may well be that a savage could not have described, or interpreted what he saw as indicating the existence of a ghost, if there had not been a justification for doing so. After all, he does not, *and cannot*, make arbitrary judgments in matters which may be of importance to his well being, and even to his life. Neither for him, nor for us would it be possible

> to choose the natural laws holding within a given degree of approximation, and in a particular set of conditions at will . . . This does not mean that we cannot, in general, make our own choices as to what we will, or will not do. But unless these choices are guided by concepts that correctly reflect the necessary relationships that exist in nature, the consequences of our actions will not in general be what we choose, but rather something different [165].

In short, every theory of the universe, whether mythological or scientific in content, possesses some degree of truth, as the choice of a false theory would lead to undesirable consequences and would therefore be at once abandoned. *Nature itself forces man to speak the truth*, and it also forces him to speak in such a way that his theories have objective reference.

This, then, is the epistemology behind Bohm's belief that every theory, however absurd it may seem at first sight, has some kind of truth in it and correctly mirrors what exists in the universe: the ill success of a theory which is outright wrong and does not describe anything whatever is a corrective which after a very short time forces us to abandon it (if we were ever foolish enough to put it forth). Knowledge is a natural process which leads to a mirroring, in the head of man, of the properties of the universe. The mirror-image may be distorted at the edges. But first of all this distortion is due to a similar objective distortion of the processes in the world. And secondly this distortion does not reach into the centre of the mirror which perfectly represents the situation at a certain level.

I do not believe that this account of our knowledge is a correct one. The simplest reason I can give for this contention of mine is that I believe man to be a little more whimsical and capricious than is assumed in the above picture of him. For in this picture it is assumed that *as a matter of fact* we recognise our mistakes, take them into account, and learn from them how to behave better. It is assumed that this process works like a well lubricated machine so that in the end whatever has been said contains some truth in it. (I suspect that a consistent elaboration of this epistemology will finally lead to the result that errors—subjective errors, that is—are never made: quite obviously Hegel's notorious 'Alles Vernünftigeist wirklich' is here lurking in the background.) But only a little knowledge of history will show that this assumption is factually false for at least two reasons. First, because there are enough examples of men, or of whole groups, who are not prepared to admit that they have been mistaken. And secondly because even death may not be a sufficient reason for changing ideas which have led to it. Quite on the contrary we often find, even in our own times, that ill success of an ill-conceived undertaking, and death resulting from it are both regarded as values and we also find the corresponding assumption that fate will sometimes deal roughly with its protegés. Furthermore, to turn to more theoretical considerations, is it not well known that refuting instances can with some ingenuity always be turned into confirming instances and that there exist elaborate theories which perform this transformation nearly automatically? Quite clearly such theories cannot be said to be in contact with reality and this in spite of their sophistication and in spite of the many fascinating statements they contain. From all this we have to conclude that *nature can never force us to admit that we have been mistaken.* Nor can it force us to recognise our mistakes. A mistake will be recognised as such only if first the conscious *decision* has been made not to make use of *ad hoc* hypotheses and to eliminate theories which do not allow of falsification. It is true that as a matter of historical fact this decision has been made by nearly all great scientists (although the present quantum theory seems to present an exception to this rule). What is of importance here is that they never were, and never could be forced to proceed in that way, either by nature or by society.

II

To sum up: at the back of Bohm's theory of knowledge there is the idea that facts and decisions both obey the same kind of laws, i.e. the laws of the material world in which we live. It is the idea that the development of moral codes, or of the laws which govern the non-moral behaviour of the members of a society, or that the development of knowledge is nothing but an aspect of the development of this material universe. This idea implies that neither the moral behaviour, not the social behaviour, nor even the status of our knowledge can be changed on the basis of an explicit decision. It is quite impossible to entertain a point of view which has no reference to any facts whatever. And it is equally impossible to introduce a new moral system unless it is somewhat related to situations already existent. This doctrine of *naturalism*[1] can be given various forms. It exists in a form which allows for the accommodation of the most revolutionary changes by simply asserting that these changes had already been prepared by the development either of the material universe or of society. In this form the doctrine is nothing but a verbal manoeuvre. Another form of the doctrine decrees that some existing pieces of knowledge, or of morals are unchangeable, because a change would amount to nothing less than a change of the unalterable course of events and of the laws which govern the universe. In this form the doctrine has very often been held by the defenders of the *status quo*. The simple logical point that decisions are never derivable from facts should show that in all its forms naturalism is based upon a logical fallacy. Now Bohm's own doctrine, although related to the doctrine of naturalism, is more detailed and less radical. He seems to admit that at times ideas may be invented which have very little to do with the facts. What he contends is, however, that these ideas will very soon be eliminated by a kind of natural selection which works either against those who hold them (they die), or against the ideas themselves (they are given up). That is, Bohm allows for deviations, but at the same time he assumes the existence of a corrective mechanism which quickly eliminates pipe-dreams and falsehoods. Now I want to show that although the doctrine in this form allows us to say that we sometimes speak the truth, it nevertheless does not give us any indication whatever as to which particular point of view expresses the truth. This we see when we ask the following important question: how long does it take this mechanism to eliminate a false hypothesis? Most certainly the length of time will depend upon the frequency with which the theory is tested, upon the decisiveness of the tests as well as upon the intention, on the side of the scientist, to take refutations seriously. Laziness and *ad hoc* manoeuvres may extend the periods of correction indefinitely. And the scientist, or whoever else is defending a certain point of view, need not perish in the course of events as he may well be careful enough to avoid tests which endanger his personal safety (there are numerous examples of this kind in the socalled ' primitive ' societies). Furthermore, who says that we shall at once stumble upon a refuting instance? But if this is so then Bohm's idea of the self-correcting character of knowledge does not help us at all to distinguish truth from falsehood. For all we know *all* our ideas may be quite thoroughly mistaken.

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[1] For an excellent discussion of this doctrine, its history, and its shortcomings see K. R. Popper, *The Open Society and Its Enemies*, Princeton, 1954, Chap. V.

Now if this is the case, and if it is further admitted that we are able to discover our errors when trying to apply the ideas we possess (provided of course, we have first *decided* to give them a form in which they are testable, and we have also decided to take refutations seriously) then the only path open to us is that we must attempt relentlessly to falsify our theories. As we do not know which part of them is true, in what domain they are true, and whether they are true at all, we must attempt the falsification under all possible conditions. Testing them under all possible conditions means assuming *first* that they are *universally valid* and *then* trying to find out the limitations of this assumption. It is this fact that we never know to what extent our theories are correct which makes us first apply them universally. If we use a theory in this way we by no means assume, as Bohm seems to think (cf. his criticism of mechanicism, discussed above) that the theory *will be found to be correct* in all domains. The universal application of a theory means rather that *we are prepared to collect refuting instances from all domains.*

The reason why I cannot accept Bohm's methodology of caution and why I prefer to it the methodology of falsification as it has been developed by Popper is therefore that the methodology of caution assumes the existence of things we know for certain, whereas I believe on the basis of the above consideration that this is much too optimistic a view of the status of our knowledge.

These, if I understand the book correctly, are the criticisms which I think must be made. But let me at once repeat that I do not therefore think the book to be of lesser value. Quite on the contrary, it is the repeated discussion and criticism of various points of view which leads to an advance of knowledge, and not the repetition of plain statements in which nobody can find any fault. To have in this way contributed to the theory of knowledge, and also to have shown the unity of (physical, philosophical etc.) knowledge is the great merit of the present book.

P. K. Feyerabend

RETROSPECTIVE MIRACLES
OR
BETTING AFTER THE RACE

There are hardly any subjects today whose status as science is seriously claimed but disputed. Whatever we may think of particular work done in the field, few would deny psychology to be a legitimate field of science. Astrology, on the other hand, is rejected for general as well as particular reasons (lack of communicable agreed procedure, extreme vagueness of predictions, etc.). The authors of the present book[1] are prominently associated with the claim for scientific status of parapsychology.

The first two chapters of this book make it pleasantly clear that there can be no doubt about the *nature* of the claim. While the definition of the subject may be awkward (embracing telepathy, clairvoyance, precognition, and psychokinesis), definition of *any* science is liable to be awkward, and perhaps not important. The authors make it abundantly clear, however, that they share the accepted notions of what constitutes a science. They do claim parapsychology to be a science.

The reader is now eager to read the evidence. Here he is disappointed. There are only some remarks (pp. 46-49; 58-63) with reference to publications that are not very easily available. Surely the subject, if genuine, is important enough to warrant the publication of a book giving the detailed evidence to date. The authors say in their foreword that answering all the questions an extremely sceptical reader might raise ' is largely of historic interest only '. The historical situation is, surely, that the majority of scientists do not believe in parapsychology. If they did, more of them would be working on it.

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[1] *Parapsychology*, by J. B. Rhine & J. G. Pratt. Oxford, Blackwell, 1957. Pp. 220. 37s. 6d.

When one comes to think of it, textbooks of accepted complete theories rarely are good source-books for those interested in evidence. A *modern* textbook of the *classical* theory of electro-magnetism will not quote many original accounts of experiments, though it should, in principle, provide a basis for experimental testing by the reader, should he be so inclined. The present reviewer feels that since parapsychology is neither generally accepted, nor complete, or even a theory at all, it is rather early to publish what is ostensibly a textbook at least in this negative respect: this book soft-pedals the evidence, and concentrates rather on definition of terms (such as precognition and telepathy) and a logical discussion of possible relations between them. In the later part of the book, there is some further discussion of methodology, and brief essays on such topics as ' parapsychology and religion '.

If the reader follows up some of the references, he will be struck, as I was, by the number of references that ultimately boil down to a discussion of the same experimental data; by the small number of ' successful ' research groups in the field (the authors may be doing their best to remedy this by this book); by the small number of successful human ' subjects ' (out of large numbers tested), and the limited periods over which even these subjects are, in fact, ' successful '.

But what constitutes ' success '? The evidence is essentially *statistical.* Such appeal to statistics as sole evidence is much rarer in the physical sciences than philosophic discussions on evaluating scientific theories might suggest. ' Significant correlation ' is used in the applied social sciences, but checked by further tests. The authors use statistics as sole means to establish the very existence of the various ' psi '-effects, such as psychokinesis (affecting the fall of dice by wishing) and precognition (predicting the order in which cards are turned up).

There is no *single* phenomenon claimed by the author that is qualitatively characterised as a ' psi '-phenomenon. It is interesting to note that there seems to be no claim of anything clearly counter-physical, such as ' dice-lifting ', only that the fall of the dice (which happens to look natural in every case) is influenced statistically by wishing. The favourite experiment consists in guessing which of five types of card is turned up. (It is not clear why the authors are using tests that are calculated to have a high random chance of success in each single event, since it is not obvious that psi effects are influenced by the number of choices.)

The authors have discovered another effect in this connection. They call it ' psi-missing '. This is a name for the case that the scoring of correct guesses is *below* the expected mean. From the amount of ' psi-missing ' the authors conclude that ' less knowledge is needed to say what a card is not than to say what it is'. Yet another effect is the ' decline ': ' High scoring subjects have always lost their ability to score above chance '.

Thus none of the strong consequences one might reasonably expect if one took parapsychology seriously, are, in fact, observed: no die (however light) is turned over ' at will '. Psychokinesis hides shyly in statistics. Again, card-guessing ability apparently does not like to become conspicuous by spectacular correct guesses out of a large choice. It prefers to just slip in unobtrusively by way of a small deviation from the mean.

There are various special explanations offered. But I believe that, when in doubt, it is fair to be guided in one's provisional judgment by certain rules: mistrust of the ' alternative plea ', of the ' ah-but ' type of argument that retains its views on shifting grounds, or, in this case, of the explaining away of difficulties by miscellaneous special *ad hoc* effects. These are some subsidiary flaws in the authors' case for parapsychology. I now come to the main and, in my opinion, fatal flaw.

It has been well said that what makes a miracle is not only *what* happens, but *when* it happens. It appears that it is not possible, so far, to predict the occurrence or kind of psi phenomenon.[1] We are left, then, with the difficult task of evaluating the significance of occurrences defined after they have occurred.

The difficulty lies in defining the basis on which the random chance is to be calculated. By analysing any list of data in sufficiently many ways *some* effect will be found arbitrarily high above ' chance ' level calculated for *that* effect only. It is very difficult to define the basis (the ' population ') fairly: should it embrace all possible psi effects, for instance, and is there such a finite set?

In many cases the authors re-analysed old data until they found ' significant ' correlation between guesses and cards turned up some draws later. Of course, the authors were frequently looking for specific effects in later experiments. Where is the line to be drawn to include unsuccessful runs at other times?

The chance of meeting any particular Mr Smith in the street may be one in a million. Yet, having in fact met him, we would hardly talk of a Psmith phenomenon. We must define both what we mean by success

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[1] ' The stage of nicely repeatable experiments that would allow the demonstration of psi effects on any or all occasions has not yet been reached. At the present point of progress such a question is not yet even in order. From this survey it should be plain, that the occurrence of psi is still largely spontaneous.' (Rhine, *New World of the Mind*, 1954.)

(meeting Mr Smith), and the population (length of time over which we propose to conduct the experiment) *before* we start the experiment. We must not select an ' interesting ' set of data and try to work out their probability. That is quite arbitrary. It is just as arbitrary to start assigning a probability to a deviation from a mean, after it has occurred, as it is to start assigning a probability to an event, after it has occurred. The operative word here is ' start '. We may indeed apply probability theory to past events. But we must define our population *before* we apply probability theory to any sample. We may not define our population *after* we have selected the sample, so as to start with the sample and perhaps include some convenient further samples. That would correspond to a highly artificial population, and must be recognised as such.

The authors do admit some of these considerations to a limited extent in their advice to new recruits (p. 171). But procedures cited as evidence (pp. 48 and 49, and again pp. 60, 63) offend even against these limited rules.

This is a beautifully produced, well-organised textbook on the fauna reported from the land of psi. A classification of animals that have eluded capture. These are strange animals, defined not by their anatomy, but by virtue of the occasions of their appearance. It is as if we were to define a halcyon as a kingfisher observed *because* the observer felt happy that day. We would never be sure that any particular kingfisher was a halcyon, and not a common kingfisher. The existence of halcyons would be based on polls showing that happy people saw more kingfishers. But if no happy person could keep up a high kingfisher-score indefinitely, we would have to be very careful in the way we collected our bird-watcher statistics. We would have to take *all* bird-watchers we ever interviewed into account together, and not concentrate on high-scoring happy ones for some days.

H. R. POST

BRAIN AND CONSCIOUSNESS

THE study of the relation of mind or consciousness to brain has traditionally been partitioned between three great disciplines—psychology, neurology, and philosophy, each of which has approached this most important of human problems from its own particular viewpoint and in each of which the discussion has been cast within definite limits that overlap to some extent but not completely. Today we are seeing something different. This partition is giving way to the activities of various individuals who have been concerned to hammer two or even all three of these accounts into a common and emergent synthesis. It is becoming more widely recognised that the relation of mind or consciousness to brain is a unitary problem and the facets it presents to psychology, neurology, and philosophy must all be taken into account if we are ever to arrive at a workable solution. However these individuals could not have attempted their task before as only comparatively recently has each subject been brought to a state of development ripe for such synthetic activity. Philosophy had to wait for the work of Broad, Ducasse, Moore, Price, and Russell, and neurology for such workers as Brain, Eccles, Schilder, Sherrington, and Sperry. The necessary insights in psychology have been gathered by such people as Ames, Klüver, Koffka and M. D. Vernon [1]. Many neurologists and psychologists have of course speculated about the mind-brain relation and have suggested various 'solutions' in recent years. But most of these solutions have died in infancy either from internal inconsistencies, a failure to grasp the complexity of the problem or an inability to analyse its essential logical features. A necessary prerequisite for any useful work in this field is an adequate grasp of the relevant parts of all the three major contributary fields of knowledge. The first work published by a neurologist to satisfy this criterion was Brain's *Mind, Perception and Science*. The book at present under review[2] is another.

Professor Kuhlenbeck's opening words are 'The relationship of brain and consciousness can be considered from the epistemological as well as from the neurological aspect'. Two-thirds of this long book are concerned with the neurological aspect. This part gives an interesting and scholarly general review of the neurology of the brain with particular attention to general principles of neural connections, problems of localisation, and of coding. The first one-third of the book is concerned with epistemology and here he is concerned mainly to support two theories in this field. The first is Russell's theory that private (perceptual) space-time is not identical with (inferred) public physical space-time, which Kuhlenbeck correctly sees to be the key to the proper solution to the whole problem. The second is the theory of psycho-neural parallelism.

He starts his analysis with a general statement of ordinary physiological materialism up to and including its account of brain. Here, however, he states 'We . . . are faced with the fundamental fact that the phenomena of consciousness exist . . .', and he claims that it is *logically* impossible to derive consciousness from material or physical processes. This is because the whole (postulated) physical world itself is based on the primary qualities

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[1] It will be noted that the work here cited is not necessarily relevant to the internal development of a subject but only to those aspects that can serve as foundations for the final synthesis.

[2] *Brain and Consciousness. Some prolegomena to an approach to the problem.* By Hartwig Kuhlenbeck, published as a supplement to Vol. 17 of *Confinia Neurologica*, 1958. Karger, Basel, 1957. Pp. iv + 344. S.Fr. 3.60

(of objects) which are merely abstracted aspects of perceptual space-time relationships ' and perceptual space-time, in turn, represents the matrix of consciousness . . . in other words : if consciousness is excluded, then space-time relationships are likewise eliminated '. This leads to the basic concept of a spatial consciousness including percepts, images, and ' a stream of internal feelings ' organised into a unit by its space-time structure. Then, rejecting the theory of psycho-neural identity on account of the well-known topological difficulties this theory involves (see Sperry),[1] he postulates, following Russell, that perceptual space-time is a *different* space-time, with a *different* set of four dimensions, to physical space-time.

He then examines idealism which he formulates as holding that only a person's perceptual space-time is actual and that physical space-time is only an abstraction. Thus there are a number of perceptual space-times belonging to different human individuals. These separate systems do not have any spatial relations to each other (as in Price's theory) and ' the physical world ' is reduced to ' a set of orderly relationships between certain events in my consciousness-field and similar events in the consciousness fields of other sentient beings [that we know of by inference] ' and the existence of any objective physical world is denied. However there are three logical fallacies here, one applicable to any formulation of idealism and two peculiar to Kuhlenbeck's own exposition, as follows :

(i) that the question of the existence of the objective physical world can be settled on logical grounds ;

(ii) because a thought or image is a mental entity, then that which the thought or image stands for or represents must also be a mental entity, or in some sense must necessarily be a fiction or logical construction.

(iii) that whereas it is legitimate to accept the inferred existence of the perceptual space-time systems of other people, yet it is illegitimate to accept the existence of the external physical world on the same basis of inference.

Kuhlenbeck himself feels the weakness of idealism when he comes to discuss the nervous system and its effect on consciousness but this is a common-sensical and not a logical qualm. However he never decides finally in favour of or against the existence of an objective physical world.

He then proceeds to discuss psycho-physical parallelism, which unwieldy system he prefers to the more straight-forward theory of interaction. This preference is based, however, on two fallacies. The first is the notion that the principle of the conservation of energy precludes the possibility of psycho-neural interaction. This argument was conclusively refuted by Broad.[2] The second is the *a priori* dictum : ' . . .events can only be causally related to each other if they occur within the same space-time system.' No arguments are presented in support of this statement which seems palpably false (see Ducasse).[1]

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[1] R. W. Sperry, ' Neurology and the Mind-brain Problem ', *Amer. Scientist*, 1952, 40, 291

[2] C. D. Broad, *The Mind and its Place in Nature*, London, 1925, pp. 103-109

A further error is contained in the reasons for Kuhlenbeck's rejection of the findings of parapsychologists (pp. 80-84). He says 'Since the formulation of guesses or of guesses disguised as wishes, and the shuffling of cards or throwing of dice are complex events, considerable deviations from a theoretical probability model may be expected to occur from time to time.' This is surely invalid. The whole point of statistics is that they can be applied to any complex phenomena, if these, or some aspect of them, can be quantified. Why is making a guess or throwing a dice any more mysteriously complex than catching measles, committing suicide, or growing prize tomatoes, to all of which and much else statistics are constantly and successfully being applied?

This is a most valuable book in spite of these small faults and much in advance of most neurological writing on the subject, which usually fails even to formulate the problem properly. Three fallacies commonly encountered in this field are (i) that the mind-brain problem cannot be solved until we have more detailed neurological (neurophysiological, neurochemical, biophysical) data. Kuhlenbeck has something sharp to say about this instance of fuzzy thinking (p. 74);[2] (ii) adhering to the statement that metaphysics is useless, dangerous, or bunk at the same time as making a great number of purely metaphysical statements about these problems; and (iii) that the mind-brain relation is an unfathomable mystery. Kuhlenbeck's book should be read by all neurologists as an antidote to these tendencies.

J. R. Smythies

Minnesota Studies in the Philosophy of Science. Vol. II. *Concepts, Theories, and the Mind-Body Problem.*
Edited by H. Feigl, M. Scriven and G. Maxwell.
University of Minnesota Press, 1958. Pp. xv + 553. $7.00 net.

The book consists of ten essays and an appendix which are arranged in two, disconnected, groups. Roughly one half of the book is about various aspects of theory construction, while the other half is taken up by discussions of the mind-body problem.

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[1] C. J. Ducasse, *Nature, Mind and Death*, La Salle, 1951

[2] Finding out more about the brain need not tell us anything about the quite different problem of brain-mind *relation*. That is to say the validity of the claim to the contrary cannot (logically cannot) be based on any empirical study of brain processes and it is always a (usually suppressed) premise to the argument.

The volume begins with an article by Oppenheim and Putnam on the Unity of Science as a Working Hypothesis. The authors argue that such a hypothesis is justified, both for reasons of methodology and of actual evidence in its favour. All scientific theories are to be ordered according to their respective ' reductive levels ' and reduced, finally, to some basic, microscopic, theory. This is neither a novel nor a striking suggestion. It all depends on what is meant by ' reduction ' and what the lowest level is to be, e.g. whether it contains only physical entities, etc. Even within physics alone such a reduction is impossible, at least today; more complex ideas such as correspondence have to be used. In the absence of any detailed method showing how the unity is achieved, this article is a mere expression of faith.

The Theoretician's Dilemma is brought about, according to Hempel, by the necessity of explaining the visible by the invisible. That is, we have to use theoretical terms and so introduce ' hypothetical entities ' in order to explain observable phenomena. It seems that, if these theoretical terms are successful in their purpose, they can be dismissed in the end and replaced by observables. This is the dilemma. Hempel concludes, however, that it is due to a mistake: theoretical terms cannot be replaced without loss by observables. I think most people would agree with this—the mistake, in my view, being created by the old-fashioned, epistemological, approach to theory construction, by the needless introduction of ontology, and so on.

This is, on the whole, familiar ground. And Scriven, writing about Definitions, Explanations, and Theories remains completely within it. His discussion as to whether reduction or definition can relate observational to theoretical terms is obscure and without issue.

The next two articles equally cover familiar ground; they are on Disposition Concepts and Extensional Logic, by Pap, and on Counterfactuals, Dispositions, and the Causal Modalities, by W. Sellars. The one concludes that extensional logic does not suffice for treating disposition concepts, while the other does not seem to arrive at any tangible conclusions.

H. G. Alexander writing about ' General Statements as Rules of Inference? ' treat this well-worn topic by merely casting doubt on the answers given by various philosophers to this question.

The second part of the book, devoted to the mind-body problem, starts with an article by Strawson on ' Persons '. He argues that ' the concept of a person is logically prior to that of an individual consciousness '. Hurrah—we have found a philosopher who admits that we are human beings before we are disembodied minds or demented bodies. This sweeps away traditional epistemology with its percepts, pains and pleasures existing by themselves in splendid isolation—but Strawson does not seem willing to follow up his argument with this conclusion.

Similarly, Zener, in an article on The Significance of Experience of the Individual for the Science of Psychology, pleads for a ' human ' and against the behaviourist view. And Meehl asks, 'When shall we use our heads instead of the formula?' This is the only article in the book which is too short. The author seems to be a practising psychiatrist and so he is aware of the genuine problems of actual science—in his case, it is the difficulty of assessing the chances whether or not a patient may benefit from treatment.

The main article in the second part of the book is by Feigl on 'The "Mental" and the "Physical"'. It is almost a book in itself—a 'supercolossal' survey of the mind-body problem. After about forty pages the author concludes that 'the central puzzle of the mind-body problem is the logical nature of the correlation laws connecting raw feel qualities with neurophysiological processes'. In other words, what is the relation of mind to brain? It takes the author another forty pages or so to claim psycho-physical identity or 'empirical synonymy' as a solution of the puzzle. He speaks also of 'two ways of knowing' the same, presumably neutral, events and illustrates his thesis by an example from physics, e.g. the relation of thermodynamics to kinetic theory. Since this is an example which I have used many years ago in the same connection (*Methodos*, 1953), I assume that the author has a similar aim, namely, to use scientific psychology rather than philosophical psychology (or epistemology) in order to clear up the mind-body puzzle. Though Feigl occasionally mentions some modern terms like cybernetics or psychosomatic medicine, he is not aware of the radical change, for instance, in the causal description, which is needed. He remains completely within the confines of epistemology, with its misleading dichotomies of mental/physical, inner/outer, direct/indirect, subjective/objective, and so on. Thus his discussion goes around in circles, without ever coming to the centre which is, after all, the human being.

This volume of 553 pages is an example of transatlantic scholarship at its worst. Too many words—and big words, at that—are chasing too few ideas. The net result is that there are too many pages to read, if one wants to find what is of value in them, and there is too much to pay for the book.

E. H. Hutten

Patterns of Discovery. By Norwood R. Hanson.
Cambridge University Press, 1958. Pp. ix + 241. 30s.
Studies in the History and Methods of the Sciences. By A. D. Ritchie.
Edinburgh University Press, 1958. Pp. 230. 12s. 6d.
The Reach of Science. By Henryk Mehlberg.
University of Toronto Press & Oxford University Press, 1958. Pp. xii + 356. 45s.

These three books are all attempts to break in, along very broad fronts, to the central citadels of the philosophy of science: i.e. to show in general terms what the essential character of the natural sciences is, and what we should require of them. Yet they attack the questions from quite different angles, and might almost be writing about different subject-matters; so that one has to stop from time to time and remind oneself that it is one and the same Science—the work of Newton and Darwin and T. H. Morgan and J. Z. Young and the rest—that they are talking about all the time.

Professor Mehlberg has, perhaps, the clearest-cut conception of what Science ought to be. His own background is in logic and 'foundational research', and the questions which for him come first are accordingly questions about verification, derivation, formulae, definitional criteria, and the like. Science, in his view, 'as of today, is the sum-total of all the special sciences as of today, that is, the sum-total of presently available solutions of any problem belonging to any special science. The bits or packages of information with their validating methods are the ultimate constituents of science;' it being understood that 'no information is scientific unless it is offered with a method of obtaining a body of evidence which adequately confirms the correctness of this information'. The paradigm of verification for him is 'direct, sensory perception', for instance of colours, and he thinks that 'there is little doubt about the fundamental role' of such perception 'in the validation of fact-like statements'. And since laws, for him, 'are bits of information expressible in single statements just as are particular facts' (such as "Phosphorus melts at 40° C"), the same basic way of establishing scientific truths will do for laws too—given the resolution of certain logical difficulties involved as a result of the passage from particular to general statements. Anyone familiar with the recent discussions set going by Tarski and Carnap will be interested to see how Professor Mehlberg puts together these (rather well-worn) building-blocks: his great merit is that of emphasising 'the lengthy and complicated inferential operations required to connect' the fundamental principles of (say) quantum mechanics with 'direct observational data' of the sort he is prepared to acknowledge.

The trouble with this approach is that it makes science appear unrealistically bitty: the well-ordered aggregate of the solutions of a vast batch of perceptual acrostics. In a word, it leaves out the *ideas* of science which (*pace* Professor Mehlberg) are born with the ambition of ruling our whole vision of the world, not just of solving piecemeal problems about items of information. And Professor Ritchie would (one fears) dismiss the whole of Mehlberg's enterprise as one more piece of misguided modern positivism. With a certain glum relish, Ritchie castigates his opponents, and defends the rights of the speculative imagination, being prepared in the last resort to argue even for the inseparability of science from religion. Visual experience provides a misleading paradigm for scientific observation, and the scientist's evidence should be thought of more on the analogy of the lawyer's or the historian's—as a way of testing whether certain general insights and ideas speak to us truthfully about the world, rather than of validating some more unit-items of information. Finally, there is no single logical problem to be solved of ' how to arrive at a general conclusion from a limited number of particular observations:' each scientist has to solve this problem by the appropriate means as he goes along, and what means are appropriate in his field is one of the things he has to find out.

In support of these general positions, Professor Ritchie takes us for some rather rambling tours of the history of science. It is an error to think of any mathematical science as purely formal: look at Euclid. The Greeks had the sense to argue about astronomy in *a priori* metaphysical terms: had they ' been imbued with the doctrines of nineteenth and twentieth century positivists the science of astronomy could hardly have begun and the pseudo-science of astrology might have come to stay'. Physical laws are about what happens ' for the most part ', as we can see from the case of Archimedes: ' human control is limited in many different ways and the notion of contingency expresses one of the aspects of that limitation'. It is illuminating to see how alike chemistry and botany seemed to earlier scientists: this is a subject Ritchie does well to rub our noses in. There could be no one ' comprehensive science of human life, character, action and so on ' and politicians should learn the lesson doctors have had to learn, that one must sometimes leave ill alone instead of ' tormenting the dying up to the last'. And in cosmology the crucial thing about Kant's antinomies is not the intellectual difficulties they create for the Hoyles and Gamows of this world: Ritchie is content to leave the first two antinomies undecided. Rather it is the practical or moral difficulties the third and fourth antinomies raise for those who have their eye on the next world: ' I can and must choose within the limits set for me by God; choice becomes both a duty and a right.' Meanwhile in politics the technocrats are upon us—Beware!

It is disappointing, to those of us who feel that the sort of analysis Professor Mehlberg presents is irrelevant to the science we have been trained in, to have sound points hinted at (as Professor Ritchie hints at them) in the context of a Jeremiad. The tradition in which Mehlberg writes leaves unexamined the very notions—*fact*, *observation*, *theory*, and the rest—which Ritchie rightly emphasises beg the crucial questions. So it is with great pleasure that one turns to Professor Hanson's original and exciting book, to find these very topics tackled head-on, and illustrated systematically with a mass of historical and psychological illustrations which do much to reinforce his logical insights. From Hanson's point of view, both Mehlberg and Ritchie would appear too one-sided. Of course all observation is theory-directed and all reports of observation are theory-loaded; but theories themselves are the outcome of thorough and methodical ratiocination. (Hanson follows up the example of Peirce, in seeking to characterise a mode of ' retroduction ' or ' abduction ' as respectable as traditional ' induction ' and 'deduction'; and interestingly enough illustrates this process with the same historical episode as Peirce described—Kepler's discovery of the elliptical orbits of the planets.) Further, there can be no single, simple logical model suitable for characterising all sorts of explanation in science—least of all the 'causal chain' model so esteemed by some philosophers. And as for attempts to give one-purpose accounts of the logical status of laws and theories: the vital thing to notice just is their multi-purpose character, which a careful historical analysis of the developing uses to which the second law of motion were put brings out very clearly. Finally, Professor Hanson launches off into a chapter of greater technicality and tries to clear up some of the confusions of thought surrounding contemporary discussions of quantum mechanics.

Hanson's book is fresh and engaging where Ritchie's is prickly and offhand and Mehlberg's a bit stodgy. He slips over from time to time into the over-jolly, but his argument is vigorous, his chief points are strong ones, and his wealth of illustrations a joy. The ideological and theological ills that Professor Ritchie denounces Hanson says nothing about; but the logical and epistemological difficulties besetting the logical empiricists he states with such force and clarity that people like Professor Mehlberg are going to have to sit up and take notice.

Stephen Toulmin

Gottfried Wilhelm Leibniz: Barock und Universalismus.
By Joachim Otto Fleckenstein.
Ott Verlag, Thun, München, 1958. Pp. 200.

Thinkers in the seventeenth century, as Herr Fleckenstein pictures them, were at once urged forward in the new directions to which Renaissance thinkers had pointed, and back to the quiet havens provided by Christian Scholasticism. The result was a series of tensions, which set the tone of philosophical and scientific thought, and which (his view involves) led to the forms in architecture, music, and painting known as Baroque. Herr Fleckenstein extends the term Baroque to cover the whole way of thinking and feeling conditioned by these tensions; so that Descartes, Spinoza, and Leibniz for him are all Baroque philosophers. I am not happy about this; but as it does not make any difference to what he says if you replace the word 'baroque' wherever it occurs by the words 'seventeenth century' (or occasionally sixteenth) I do not propose to discuss it.

The main tension is between what he describes as two distinct ways of thinking—though I should prefer to describe them as different emphases of thought—predicative and functional. For the predicative thinker the important thing is to discover what characteristics are possessed by the things in which he is interested, whereas what the functional thinker is interested in are the laws relating to the succession or the concomitance of characteristics, the substances to which these characteristics belong being relatively unimportant. Galileo's law of falling bodies is an early example of functional thinking, and the stress on mathematics, and the conviction that the fundamental laws of nature were laws of motion, made this kind of thinking essential for the study of actual occurrences. But seventeenth-century thinkers were anxious, not merely to believe, but to understand the doctrines of Christianity, and the only intellectual approach to these doctrines seemed to be through predicative thinking. Herr Fleckenstein plays these two ' kinds ' of thinking against one another very cleverly in his account (which I think is far too simple) of the transition from medieval to modern thought, and in his account of the way in which Leibniz succeeds in harmonising them better than any other thinker of the century; and his interpretation of the *Monadology* in the light of the mathematical conception of an analytic function is extremely ingenious and suggestive. His treatment of Leibniz in the light of the stresses and tensions of the seventeenth century is very clear, lively, and interesting. He is not a specialist in Leibniz, and he relies mainly on the German translations in the selection made by Buchenau and Cassirer, but he is very intelligent, though not always trustworthy. Sometimes he ekes out a scanty knowledge of the details of Leibniz's writings by speculations about Leibniz's thinking, and on a number of occasions he misinterprets the documents he quotes (e.g. Leibniz's letter to Gabriel Wagner, 1696, which actually shows Leibniz's early dependence on Aristotle's logic, and which Herr Fleckenstein interprets (86) as showing ' the Cartesian root ' of Leibniz's thought). In his account of the monads he speaks (118) as if he thought that Leibniz's ' substantial forms ' were themselves monads, whereas a monad is not merely form but also matter; again, he lays too much stress on relations, and even (140) speaks of the reality of the monads as consisting only in their relations to one another—which is to say the least very misleading, though he had spoken rather more carefully on page 34. His discussion of acceleration (100) in relation to force involves the attribution to Leibniz of an insight which was reserved for Newton: Leibniz never measured force by rate of change of anything; he always measured the force of a moving body by the effects it could produce before being brought to rest. There are a number of minor oversights: the *Monadology* was not published in Leibniz's lifetime (123); the *Discourse on Metaphysics* was not written in the course of a discussion with Arnauld.

But in spite of these minor blemishes, the book is on the whole brilliantly written and contains a great deal of interesting matter.

Herr Fleckenstein is a privat-docent in the department of Astronomy and Meteorology in the University of Basel. This is his second book on the history of thought; his first was on Euler.

L. J. Russell

Seekers and Finders, Books 1, 2, and 3. By Amabel Williams-Ellis. Blackie & Sons, 1958. 6s. each.

> 'I do wonder if the world would ever have got on at all if there hadn't always been children to make grown-up people go on thinking and wanting to know. . . . Children *ask*: . . .'.

I WAS prepared to review these attractive-looking books because it seemed so laudable that someone should take the trouble to discuss scientific method respectfully with children. So much of scientific method is an attitude of mind that grows from childhood onwards and can only with great difficulty be inculcated at a later age, that someone who is willing and able to contribute verbally at the beginning of the process should be hailed with gratitude. What I was not expecting to find and what makes the whole so refreshing is the other side of the penny: recognition of the fact that children play a vital part in stimulating the scientific thinking of adults and are a constant reminder that experience comes new to all of us and that the final answer is never known.

If this sounds trite and obvious, the fault is mine and not that of the writer. Mrs Williams-Ellis is able to convey the excitement and importance of her subject matter with accurate simplicity and enjoyment. Few enough are the scientist-journalists who can popularise science successfully; to succeed in doing this for children without distortion or whimsy is even rarer.

I recommend that all who have any excuse for giving a present to a child between the ages of six and twelve choose this series without hesitation, but do make sure of buying them in time to read them yourself first.

Book I (*They dared to Ask Questions*, pp. 68) illustrates the difference between science and magic and between scientists and inventors. The romance of Pasteur's, Darwin's, and others' stories is recaptured, but the common theme is never lost. The illustrations in it, by Geoffrey Fraser and Laurence Bradbury, are particularly good.

Book II (*Magic, Science and Invention*, pp. 72) continues the theme and the lessons emerge effortlessly and attractively: look, listen, record, compare; make controlled experiments; never jump to conclusions. Doing what you like doing without too much money is also recommended as fun and a stimulant to scientific progress!

Book III (*You Yourself*, pp. 64) is an unorthodox introduction to human anatomy and physiology in question and answer form. Where do babies come from ? is *not* one of the questions.

There are three further books in the series.

JOAN M. HUTTEN

ANNOUNCEMENT

Meetings of the British Society for the Philosophy of Science

(Philosophy of Science Group up to 2 March)

The following meetings were held during the academic year, 1958-59. They took place, by kind permission, in the Joint Staff Common-Room, University College, London.

1958

6 October: Chairman's Address (Professor R. O. Kapp), ' Space '

10 November: Professor R. B. Braithwaite, ' Identifications as the only Axioms for a Science '

1 December: Dr R. Peters, ' A Defence of Aristotelianism in Psychology '

1959

12 January: Dr F. Waismann, ' Logic and Reality '

9 February: Professor P. G. 'Espinasse, ' The Concept of the Gene '

2 March: Annual General Meeting. Dr W. Grey Walter, ' The Study of Complex Systems '

20 April: Dr W. C. Salmon, ' The Concept of Empirical Verifiability '

11 May: Mr G. Gorer, ' The Concept of National Character '

15 June: Dr D. Bohm, ' The Interpretation of Quantum Theory '

NOTICES

The British Society of Rheology will hold a meeting in Manchester on 7th and 8th April, 1960. Part of it will be devoted to the methodology of the subject. Full details can be obtained from the Honorary Secretary, 8 The Broadway, Pontypool, Mon.

Philosophy of Science Conference

The Fifth Annual Conference of the British Society for the Philosophy of Science will be held at Wills Hill, University of Bristol, from 23 to 25 September, 1960. Details will be obtainable from Dr. Mary B. Hesse, Whipple Science Museum, Free School Lane, Cambridge.